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THE PSYCHOLOGICAL REVIEW

STATISTICAL THEORY OF SPONTANEOUS RECOVERY AND REGRESSION

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From the viewpoint of one interested in constructing a learning theory, it would be convenient if an organism's habits of responding with respect to any given situation were modifiable only during periods of exposure to the situation. In that case, it would not be unreasonable, prima facie, to hope that all of the empirical laws of learning could be stated in terms of relations between behavioral and environmental variables. Nothing in psychology is much more certain, however, than that orderly changes in response tendencies—e.g., spontaneous recovery, forgetting—do occur during intervals when the organism and the situation are well separated.

How are these "spontaneous" changes to be accounted for? It is easy enough to construct a law expressing some behavioral measure as a function of time, but an unfilled temporal interval never remains permanently satisfying as an explanatory variable. The temporal gap has to be filled with events of some sort, observed or inferred, in the environment or in the organism. The favorite candidate for the intervening position has usually been a postulated state or process, either neural or purely hypothetical, which varies spontaneously during rest intervals in whatever manner is required to account for the behavioral changes. The difficulty with this type of construct is that it is always much easier to postulate than to unpostulate. Few hypothetical entities are so ill-favored that once having secured a foothold they cannot face out each new turn of empirical events with the aid of a few *ad hoc* assumptions.

The approach to time-dependent learning phenomena which will be illustrated in this paper attempts to shift the burden of explanation from hypothesized processes in the organism to statistical properties of environmental events. The very extensiveness of the array of hypothetical constructs—e.g., set, reactive inhibition, memory trace—which now compete for attention in this area suggests that postulates of this type have entered the scene prematurely. Until more parsimonious explanatory variables have been fully explored, it will scarcely be possible either to define clearly the class of problems which require explanation or to evaluate the various special hypotheses that have been proposed.

By "more parsimonious" sources of explanation, I refer to the variables, ordinarily stimulus variables, which are intrinsic to a given type of behavioral situation and thus must be expected to play a role in any interpretive schema.

[1] This paper was prepared during the author's tenure as a faculty research fellow of the Social Science Research Council.

In the present instance we are interested specifically in the way learned response tendencies change during rest intervals following experimental periods. And we note that there are two principal ways in which stimulus variables could lead to modification in response tendencies during rest intervals. The first is the direct effect that changes in the stimulus characteristics of experimental situations from trial to trial or period to period may have upon response probability. The second is the learning that may occur between periods if the stimulating situations obtaining within and between periods have elements in common. The former category can again be subdivided according as the environmental variation is systematic or random.

The random component has been selected as our first subject of investigation for several reasons. One is that it has received little attention heretofore in learning theory. Another is that in other sciences apparently spontaneous changes in observables have frequently turned out to be attributable to random processes at a more molecular level. Perhaps not surprisingly, considerable analysis has been needed in order to ascertain how random environmental fluctuations during intervals of rest following learning periods would be expected to influence response probabilities. It will require the remainder of this paper to summarize the methods and results of this one phase of the over-all investigation.

GENERAL THEORY OF STIMULUS FLUCTUATION

Even prior to a detailed analysis, we can anticipate that whenever environmental fluctuation occurs, the probability of a response at the end of one experimental period will not be the same as the probability at the beginning of the next. If conditioning is carried out during a given period, some of the newly conditioned stimulus elements [2] will be replaced before the next period by elements which have not previously been available for conditioning. Similarly, during the interval following an extinction period, random fluctuation will lead to the replacement of some of the just extinguished stimulus elements by others which were sampled during conditioning but have not been available during extinction. In either case, the result will be a progressive change in response probability as a function of duration of the rest interval.

In order to make these ideas testable, we must state more formally and explicitly the concepts and assumptions involved. Once this is done, we will have in effect a fragmentary theory, or model, which may account for certain apparently spontaneous changes in response tendencies. At a minimum, this formal model will enable us to derive the logical consequences of the concept of random environmental fluctuation so that they may be tested against experimental data. If the correspondence turns out to be good, we may wish to incorporate this model into the conceptual structure of S-R learning theory, viewing it as a limited theory which accounts for a specific class of time-dependent phenomena.

Most of the assumptions we shall require have been discussed elsewhere (8) and need only be restated briefly for our present purposes.

a. Any environmental situation, as constituted at a given time, determines for a given organism a population of

[2] For reasons of mathematical simplicity and convenience I shall develop these ideas in terms of the concepts of statistical learning theory. It will be apparent, however, that within the Hullian system a similar argument could be worked out in terms of the fluctuation of stimuli along generalization continua.

stimulus events from which a sample affects the organism's behavior at any instant; in statistical learning theories the population is conceptualized as a set of stimulus elements from which a random sample is drawn on each trial.

b. Conditioning and extinction occur only with respect to the elements sampled on a trial.

c. The behaviors available to an organism in a given situation may be categorized into mutually exclusive and exhaustive response classes.

d. At any time, each stimulus element in the population is conditioned to exactly one of these response classes.

On the basis of these assumptions, functions have been derived by various investigators (2, 5, 8, 16, 21) to describe the course of learning predicted for an idealized situation in which the physical environment is perfectly constant and the organism samples the stimulus population on each trial. No idealized situations are available for testing purposes, but the theory seems to give good approximations to empirical learning functions obtained in short experimental periods under well-controlled conditions.

In the present paper we turn our attention from behavioral changes that occur within experimental periods to the changes that occur as a function of the intervals between periods. Correspondingly, we replace the simplifying assumption of a perfectly constant situation with the assumption of a randomly fluctuating situation.[3] Specifically, it will be assumed that the availability of stimulus elements during a given learning period depends upon a large number of independently variable components or aspects of the environmental situation, all of which undergo constant random fluctuation.

Now let us consider the type of experiment in which an organism is run for more than one period in the same apparatus. In dealing with the behavior that occurs during any given experimental period, the total population S_* of stimulus elements available in the situation at any time during the experiment can be partitioned into two portions: the subset S of elements which are available during that period and the subset S' of elements which are not. Under the conditions considered in this paper, the probability of a response at any given time during the period is equal to the proportion of elements in the available set S that are conditioned to that response. Owing to environmental fluctuation, there is some probability j that an element in the available set S will become unavailable, i.e., go into S', during any given interval Δt, and a probability j' that an element in S' will enter S. These ideas are illustrated in Fig. 1 for a hypothetical situation.

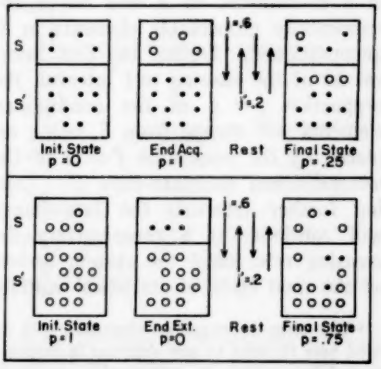


FIG. 1. Fluctuations in stimulus sets during spontaneous regression (upper panel) and spontaneous recovery from extinction (lower panel). Circles represent elements connected to response A. Values of p represent probabilities of response A in the available set S.

[3] It is possible now to go back and "correct" the functions derived earlier to allow for this random variation, but we will not be able to go into this point in the present paper.

The relevance of the scheme for learning phenomena arises from the fact that both conditioned and unconditioned elements will constantly be fluctuating in and out of the available set S. During an experimental period in which conditioning or extinction occurs, the proportion of conditioned elements in S will increase or decrease relative to the proportion in S'. But during a subsequent rest interval, these proportions will tend toward equality as a result of the fluctuation process.

INTERPRETATION OF SPONTANEOUS RECOVERY AND REGRESSION [4]

The essentials of our treatment of spontaneous recovery and regression will be clear from an inspection of Fig. 1. The upper panel illustrates a case in which, starting from a zero level, conditioning of a given response A is carried out during one period until the probability of A in the available situation represented by the set S is unity. At the end of the conditioning period we will have, neglecting any fluctuation that may have occurred during the period, all of the elements in S conditioned to A and all of the temporarily unavailable elements in S' unconditioned. During the first interval Δt of the ensuing rest interval, the proportion $j = .6$ of the conditioned elements will escape from S, being replaced by the proportion $j' = .2$ of the unconditioned elements from S'. During further intervals the interchange will continue, at a progressively decreasing rate, until the system arrives at the final state of statistical equilibrium in which the densities of conditioned elements in S and S' are equal. The predicted course of spontaneous regression in terms of the proportion of conditioned elements that will be in S at any time following the conditioning period is given by the topmost curve in the upper panel of Fig. 2. The equation of the curve will be derived in a later section.

In an analogous fashion the essentials of the spontaneous recovery process are schematized in the lower panel of Fig. 1. We begin at the left with a situation following maximal conditioning so that all elements are conditioned to response A. During a single period of extinction, all elements in the available set S are conditioned to the class of competing responses $\bar{A}$ and the probability of A goes temporarily to zero. Then during a recovery interval, the random interchange of conditioned and unconditioned elements between S and S' results in a gradual increase in the proportion of conditioned elements in S until the final equilibrium state is reached. The predicted course of spontaneous recovery as a function of time is given by the topmost curve in the lower panel of Fig. 3.

According to this analysis, spontaneous regression and recovery are to be regarded as two aspects of the same process. In each case the form of the process is given by a negatively accelerated curve with the relative rate of change depending solely upon the characteristics of the physical situation embodied in the parameters j and j'. Rates of regression and recovery should, then, vary together whenever the variability of the stimulating situation is modified.

It cannot be assumed, however, that amounts of regression and recovery should be equal and opposite in all experiments. The illustrative example of Fig. 1 meets two special conditions that do not always hold: (*a*) the condition-

[4] The term *spontaneous regression* will be used here to refer to any decrease in response probability which is attributable solely to stimulus fluctuation. It is assumed that over short time intervals, the empirical phenomenon of forgetting may be virtually identified with regression, but that over longer intervals forgetting is influenced to an increasing extent by effects of interpolated learning.

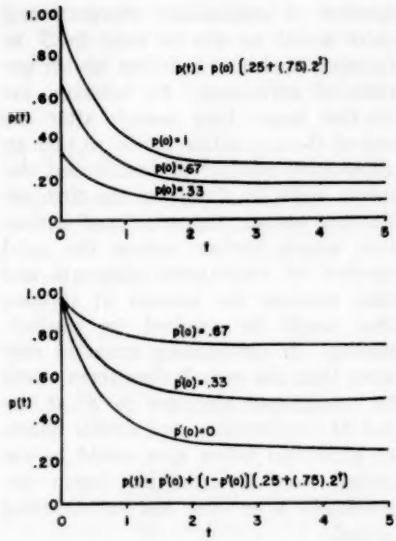


FIG. 2. Families of spontaneous regression curves. In the upper panel the proportion of conditioned elements in S' at the end of conditioning is zero and the proportion in S is the parameter. In the lower panel the proportion of conditioned elements in S at the end of conditioning is unity and the proportion in S' is the parameter.

ing and extinction series start from initial response probabilities of zero and unity, respectively; and (*b*) conditioning and extinction are carried to comparable criteria within the experimental period preceding the rest interval.

PREDICTIONS CONCERNING EFFECTS OF EXPERIMENTAL VARIABLES

Terminal level of conditioning or extinction. If other conditions remain fixed, the level of response probability attained at the end of a single learning period will determine both the initial value and the asymptote of the curve of regression or recovery. For the situation represented by the upper panel of Fig. 1, the curve of conditioning goes to unity, and the predicted course of spontaneous regression is given by the top curve in the upper panel of Fig. 2. If in the same situation, conditioning has been carried only to a probability level of, say, .67, then the total number of conditioned elements will be smaller and the curve of regression will not only start at a lower value, but will run to a lower asymptote, and so on. Similarly, if in the situation represented by the lower panel of Fig. 1, response probability goes to zero during the extinction period, the predicted course of spontaneous recovery is given by the lowest curve in the upper panel of Fig. 3; if extinction terminates at higher probability levels, we obtain the successively higher recovery curves shown in the figure.

Number of preceding learning periods. Increasing the number of preceding acquisition periods would tend to increase the total number of condi-

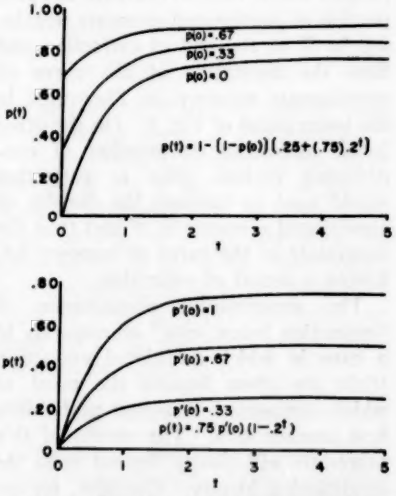


FIG. 3. Families of spontaneous recovery curves. In the upper panel the proportion of conditioned elements in S' at the end of extinction is unity and the proportion of conditioned elements in S at the end of extinction is the parameter. In the lower panel, the proportion of conditioned elements in S' at the end of extinction is the parameter and the proportion in S is zero.

tioned elements in S_* and therefore the asymptote of the curve of regression. If level of response probability at the end of the last acquisition period is fixed at some one value, say unity, then variation in the proportion of conditioned elements in S' yields the family of regression curves illustrated in the lower panel of Fig. 2, all curves starting at the same point but diverging to different asymptotes. This curve family will be recognized as corresponding to the well-known relationship between retention and amount of overlearning, where overlearning is defined in terms of additional training beyond the point at which response probability in the temporarily available situation reaches unity.

Analogous considerations apply in the case of spontaneous recovery. Increasing the number of preceding extinction periods would tend to decrease the proportion of conditioned elements remaining in S' at the end of extinction and thus the asymptote of the curve of spontaneous recovery, as illustrated in the lower panel of Fig. 3. On the other hand, increasing the number of conditioning periods prior to extinction would tend to increase the density of conditioned elements in S' and thus the asymptote of the curve of recovery following a period of extinction.

The experimental phenomenon of "extinction below zero" corresponds to a case in which additional extinction trials are given beyond the point at which temporary response probability first reaches zero. The results of this procedure will clearly depend upon the conditioning history. Consider, for example, the situation illustrated in the top row of Fig. 1. If extinction were begun immediately following the conditioning period, then we would expect extinction below zero to have little effect, for at the end of the first extinction period the set S would be exhausted of conditioned elements and there would be few or none in S' to fluctuate back into S during further periods of extinction. If, however, extinction began long enough after the end of the acquisition period so that an appreciable number of conditioned elements were in S' during the first extinction period, the additional extinction would further reduce the total number of conditioned elements and thus increase the amount of training that would be required for reconditioning. If conditioning extended over more than one period, then there would be conditioned elements in S' at the end of conditioning, and similar effects of extinction below zero would be expected even if extinction began immediately after the last conditioning period.

Distribution of practice. In general, amount of spontaneous regression should vary inversely with duration of the intertrial interval during conditioning, and spontaneous recovery should vary inversely with duration of the intertrial interval during extinction. In each case, the length of the intertrial interval will determine the extent to which the stimulating situation can change between trials, and thus the proportion of the elements in the stimulus population S_* which will be sampled during a given number of trials. These relationships will be treated in more detail in a forthcoming paper (7).

MATHEMATICAL DEVELOPMENT OF FLUCTUATION THEORY

Stimulus fluctuation model. Let the probability that any given element of a total set S_* is in the available set S at time t be represented by $f(t)$, the probability that an element in S escapes into the unavailable set S' during a time interval Δt by j, and the probability that an element in S' enters S during an interval Δt by j'. Then by

elementary probability theory we have for the probability that an element is in S at the end of the $(t+1)$st interval Δt following an experimental period:

$$f(t+1) = [1-f(t)]j' + f(t)(1-j).$$

This difference equation can be solved by standard methods (2, 12) to yield a formula for $f(t)$ in terms of t and the parameters:

$$f(t) = \frac{j'}{j+j'} - \left[\frac{j'}{j+j'} - f(0)\right](1-j-j')^t$$
$$= J - [J-f(0)]a^t \qquad [1]$$

where $f(0)$ is the initial value of $f(t)$; J represents the fraction $j'/j+j'$; and a represents the quantity $(1-j-j')$. Since a is bounded between -1 and $+1$ by the definition of j and j', the probability that any element[5] is in S will settle down to the constant value J after a sufficiently long interval of time, and the total numbers of elements in S and S' will stabilize at mean values N and N', respectively, which satisfy the relation.

$$N = J(N+N'). \qquad [2]$$

Spontaneous recovery and regression. Curves of spontaneous recovery and regression can now be obtained by appropriate application of Equation 1. Let us designate by $p(t)$ and $p'(t)$ the proportions of conditioned elements, and therefore the response probabilities, in S and S' respectively at time t following an experimental period. The set of conditioned elements in S at time t will come in part from the conditioned elements, $p(0)N$ in number, that were in S at the end of the experimental period, and in part from the conditioned elements, $p'(0)N'$ in number, that were in S'. The probabilities of finding elements from these two sources in S at time t are obtained from Equation 1 by setting $f(0)$ equal to 1 and 0 respectively. With these relations at hand we are ready to write the general expression for spontaneous recovery and regression:

$$p(t) = \frac{1}{N}[p(0)\{J-(J-1)a^t\}N + p'(0)J(1-a^t)N']$$
$$= p(0)[J-(J-1)a^t] + p'(0)(1-a^t)(1-J), \qquad [3]$$

the parameters N and N' having been eliminated by means of Equation 2.

The functions illustrated by the curve families of Fig. 2 and 3 are all special cases of Equation 3. In the upper panel of Fig. 2, $p'(0)$ has been set equal to 0; in the lower panel, $p(0)$ has been set equal to 1. In the upper panel of Fig. 3, $p'(0)$ has been set equal to 1; in the lower panel, $p(0)$ has been set equal to 0.

EMPIRICAL RELEVANCE AND ADEQUACY

General considerations. The theoretical developments of the preceding sections present two aspects, one general and one specific, which are by no means on the same footing with regard to testability. It will be necessary to discuss separately the general concept of stimulus fluctuation and the specific mathematical model utilized for pur-

[5] For simplicity, it has been assumed in this paper that all of the elements in S_a have the same values of j and j'. In dealing with some situations it might be more reasonable to assume that different parameter values are associated with different elements. For example, data obtained by Homme (11) suggest that in the Skinner box a portion of the elements should be regarded as fixed and always available while the remainder fluctuate. Application of an analytic method described elsewhere (8) shows that conclusions in the general case will differ only quantitatively from those given in this paper.

poses of deriving its testable consequences.

The reason why the fluctuation concept had to be incorporated into a formal theory in order to be tested was, of course, the difficulty of direct observational check. Thus for the present this concept must be treated with the same reserve and even suspicion as any interpretation which appeals to unobservable events. This remoteness from direct observation may, however, represent only a transitory stage in the development of the theory. Relatively direct attacks upon certain aspects of the stimulus element concept are provided by recent experiments (1, 21) in which the sampling of stimulus populations has been modified experimentally and the outcome compared with theoretical expectation. Further, it should be noted that the idea of stimulus fluctuation is well grounded in physical considerations. Surely no one would deny that stimulus fluctuation must occur continuously; the only question is whether fluctuations are large enough under ordinary experimental conditions to yield detectable effects upon behavior. The surmise that they are is not a new one; the idea of fluctuating environmental components has been used in an explanatory sense by a number of investigators in connection with particular problems: e.g., by Pavlov (19) and Skinner (22) in accounting for perturbations in curves of conditioning or extinction, by Guthrie (10) in accounting for the effects of repetition, and recently by Saltz (20) in accounting for disinhibition and reminiscence.

Considered in isolation, the concept of stimulus fluctuation is not even indirectly testable; it must be incorporated into some broader body of theory before empirical consequences can be derived. In the present paper we have found that when this concept is taken in conjunction with other concepts and assumptions common to contemporary statistical learning theories (2, 5, 8, 16), the result of the union is a mathematical model which yields a large number of predictions concerning changes in response probability during rest intervals. Once formulated, this model is readily subject to experimental test. Its adequacy as a descriptive theory of spontaneous recovery and regression can be evaluated quite independently of the merits of the underlying idea of stimulus fluctuation.

Spontaneous recovery. Space does not permit the detailed discussion of experimental studies, and we shall have to limit ourselves to a brief summary of empirical relationships derivable from the theory, together with appropriate references to the experimental literature. To the best of my knowledge, the references cited include all studies which provide quantitative data suitable for comparison with predicted functions.

a. The curve of recovery is exponential in form (3, 9, 17) with the slope independent of the initial value (3).

b. The asymptote of recovery is inversely related to the degree of extinction (3, 11).

c. The asymptote of recovery is directly related to the number of conditioning periods given prior to extinction (11).

d. The asymptote of recovery is directly related to the spacing of preceding conditioning periods (11).

e. Amount of recovery progressively decreases during a series of successive extinction periods (4; 13; 19, p. 61).

It may be noted that items *c* and *d* represent empirical findings growing out of a study conducted expressly to test certain aspects of the theory. Many additional predictions derivable from the theory must remain unevaluated until appropriate experimental evidence becomes available, e.g., the inverse relation between asymptote of recovery and spacing of extinction trials or periods, and the predictions concerning "ex-

tinction below zero" mentioned in a previous section.

Spontaneous regression. Predictions concerning functional relationships between spontaneous regression and such experimental variables as trial spacing or degree of learning parallel those given above for spontaneous recovery, but in the case of regression there are fewer data available for purposes of verification. The predicted exponential decrease in amount of regression as a function of number of preceding learning periods has been observed in several studies (**6**, **11**, **13**, **14**). Predictions concerning regression in relation to spacing of learning periods have not been tested in conditioning situations, but they seem to be in agreement with rather widely established empirical relationships between spacing and retention in human learning (**15**, pp. 156–158; **18**, p. 508).

Finally, the question may be raised whether there are no experimental facts that would embarrass the present theory. If a claim of comprehensiveness had been made for the theory, then negative instances would be abundantly available. Under some conditions, for example, recovery or regression fails to appear at all following extinction or conditioning, respectively. Since, however, we are dealing with a theory that is limited to effects of a single independent variable, stimulus fluctuation, instances of that sort are of no special significance. Like any limited theory, this one can be tested only in situations where suitable measures are taken and where the effects of variables not represented in the model are either negligible or else quantitatively predictable. And subject to these qualifications, available evidence seems to be uniformly confirmatory. The danger of continually evading negative evidence by *ad hoc* appeals to other variables cannot be entirely obviated, but it may be progressively reduced if we are successful in bringing other relevant independent variables into the theoretical fold by further applications of the analytical method illustrated here.

SUMMARY

In this paper we have investigated the possibility that certain apparently spontaneous behavioral changes, e.g., recovery from extinction, may be accounted for in terms of random fluctuation in stimulus conditions. Taken in isolation, the concept of random stimulus fluctuation has proved untestable, but when incorporated into a model it has led to quantitative descriptions of a variety of already established empirical relationships concerning spontaneous recovery and regression and to the determination of some new ones. A forthcoming paper in which the same model is applied to the problem of distribution of practice will provide further evaluation of its scope and usefulness in the interpretation of learning phenomena.

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THE CONSTANCY OF THE *I–V*: A CRITIQUE OF INTERVENING VARIABLES

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In the last 25 years American psychologists have become acutely theory-conscious. Developments in physics and philosophy are partly responsible for this state of mind. Operationists and logical empiricists have taught us how "free-floating" constructs can generate pseudo problems and vicious circles. Turning a critical eye on our own scientific activities we have found such dangers not exaggerated.

Psychologists tend to react in either of two ways. One group, the positivists, barricades itself against error by establishing a line of defense at the level of empirical generalization. Brunswik (**3**), for example, would keep the organism empty and devote his efforts to correlating its behavioral adjustments with changes in the environment. A second group, the systematists, finds the atmosphere stimulating to greater theoretical effort. Logical criticism has merely made them look more closely to their conceptual weapons and prepare for a longer campaign. Tolman (**30**) pointed the way with his floor plan for operational behaviorism; Hull (**12, 15, 16**) put the plan to work in building his own system.

The gist of the Tolman-Hull method is too well known to need detailed description here. Essentially, it involves setting up a number of hypothetical constructs, or intervening variables (*I-V*),[1] between stimulus variables (*S-V*) and response variables (*R-V*). Each *I-V* is especially sensitive to a single external or internal condition; combined, they give the organism's behavior. The mathematical function relating a response to its many determining stimuli can thus be broken down into a number of simpler components. It should be noted that there are no gaps in this hypothetical chain. In principle, there is no quarrel between the positivist Skinner and the Hullians. His objections to theories of learning are aimed explicitly at statements concerning "events taking place . . . at some other level of observation and measured, if at all, in different dimensions" (**26**, p. 193). In this sense Hull and Tolman are no theorists. Their *I-V*'s belong on another level of abstraction, it is true, but they refer to the same level of observation.

The issue between positivist and systematist is pragmatic. Does the *I-V* pay off? It is the purpose of the present article to examine the arguments for this type of theory in the light of recent evidence.

THE CASE FOR *I-V*

Intervening variables appear to have two main functions in behavior theory: heuristic and formal. The first function precedes the second and is most fully revealed in Tolman's writings. A construct such as habit, drive, expectation, or demand may help to suggest new relationships to be sought between *S-V*'s and *R-V*'s. It may do so, however, without appearing in the final equation, $R\text{-}V = F(S\text{-}V)$. Hull's (**13**, p. 285) argument for a construct of habit based on

[1] The terms are used interchangeably here, although possible distinctions between them may prove useful for other purposes (**8, 22**).

the time gap between acquisition and use belongs in this category.

Since the heuristic role, vital as it is, can be played behind the scenes, we are here concerned with the formal function of the *I-V*. What use can the theorist make of it in integrating experimental results? Bergmann and Spence (**2**) and Hull (**13**) answer that it is a device for simplifying the otherwise baffling mathematical relationships underlying empirical curves. Just how is this simplification brought about and to what end?

First let us consider a single *R-V*; e.g., the speed of a rat in a runway. We suppose this measure to depend on a variety of conditions: hours of deprivation, amount of reward, length of runway, number of previous trials, pattern of reinforcements, and others as yet unspecified.[2] Our problem is, given certain values of these variables, to predict the rat's running speed. To solve this problem Tolman (**30**) suggests that we vary one *S-V* at a time, holding all others constant, and measure the variations in speed that result. This gives us a set of mathematical functions each relating speed to some single *S-V*. We take each function to define an *I-V* and assume that it remains constant for different values of the other *S-V*'s. On this assumption, although Tolman hesitates to draw the inference, we can presumably combine the several functions by some simple rule—e.g., addition or multiplication—to derive *R-V*.

If the theorist is permitted such an assumption he can hardly deny the empiricist the same privilege. A valid objection, however, is that he begs the very question he set out to answer: how do the *S-V*'s combine? To test the assumption that they do not interact he must vary them not only one at a time but jointly. Such tests may lead to more complicated functions expressing the interaction of two or more *S-V*'s. The point is that in carrying out these tests the system builder will have made as many determinations as the positivist. True, he may be able to express the results in a series of simpler equations, but he can combine them to predict only the outcomes already discovered. As long as he confines himself to a single *R-V* it looks as if the investigator can get along without any formal help from *I-V*'s. Spence (**27**) recognized this point and suggested that it might explain Skinner's lack of interest in theory construction. Simplification by itself is not enough to justify the extra effort. The question is whether these simplified expressions can be put to use.

It is time to face the crux of the matter. What does it mean to ask if an *I-V* "pays off," if it can be "put to use"? The real question is: does it enable us to formulate theorems from which behavior can be deduced? Can we, with the aid of *I-V*'s, hope to predict *S-R* relationships that have never actually been observed? By analogy with the physical sciences it might seem that a truly deductive theory must describe an order of events other than the one to be explained. If this were true, prediction of molar outcomes, aside from simple generalization, would have to be based on molecular laws. Skinner would be right and the systematist should either study physiology or collect many facts. If it is *not* true, how can the *I-V*, without providing more than a vicarious foothold on the molecular substratum, lead to bona fide prediction?

[2] To call "number of trials" or "pattern of reinforcements" a stimulus variable may seem unwarranted. As here used the term is synonymous with the more familiar "independent variable" of the laboratory; it is introduced solely for convenience.

So far we have considered but one *R-V*. Adding a second *R-V* changes the picture. As Feigl (6) pointed out, if we have m causes and n effects and take a pair at a time, there are mn possible relations to be discovered. By inserting an *I-V* and finding its relation to each cause and effect in turn, we change the number of determinations to $m + n$. This change is a reduction in all cases except where m or $n = 1$ or where $m = n = 2$. Let us assume, for example, three *S-V*'s and two *R-V*'s. To establish a simple relation between each *S-V* and each *R-V* requires six determinations. The same situation with one *I-V* is shown in Fig. 1A. The number of simple relations to be determined is reduced from six to five. As more *S-V*'s or *R-V*'s are added, the amount of reduction increases at an accelerating rate.[3]

This would be all very well, the reader may point out, if *I-V* could be directly observed. Since it cannot, its relations can only be inferred from measurements of some *R-V* as a function of some *S-V*. How, then, can any saving be effected? To answer this question refer to Fig. 1E. Let F stand for any function relating an *R-V* directly to an *S-V*. Suppose we wish to find $R\text{-}V_1 = F_{12}(S\text{-}V_2)$ without having to measure it. One method would be as follows:

Step 1. Measure $R\text{-}V_1$ for different values of $S\text{-}V_1$ and find

$$R\text{-}V_1 = F_1(S\text{-}V_1). \qquad [1]$$

Step 2. By logical and mathematical procedures derive

$$I\text{-}V = \phi_1(R\text{-}V_1) \qquad [2]$$

and

$$I\text{-}V = f_1(S\text{-}V_1). \qquad [3]$$

Step 3. Now measure $R\text{-}V_2$ for different values of $S\text{-}V_2$ and find

$$R\text{-}V_2 = F_2(S\text{-}V_2). \qquad [4]$$

Step 4. In like manner derive

$$I\text{-}V = \phi_2(R\text{-}V_2) \qquad [5]$$

and

$$I\text{-}V = f_2(S\text{-}V_2). \qquad [6]$$

Step 5. By combining equations 2 and 6 algebraically, or, as an approximation, by entering numerical values and fitting a curve, we arrive at a statement of $R\text{-}V_1 = F_{12}(S\text{-}V_2)$.

If in Step 5 equations 3 and 5 were used instead of 2 and 6, the result would be $R\text{-}V_2 = F_{21}(S\text{-}V_1)$. Note that from two sets of experimental observations we can write four *S-R* functions. If the method is sound it appears that even a single *I-V* can give the scientist something of the predictive power he seeks.

But the method's soundness depends on a basic assumption: that *I-V* remains the same whether derived from one set of *S-R* variables or another. This principle we may call the *constancy of the* I-V.

What does it mean to say that an *I-V* is constant? Even if a corresponding physical mechanism exists we have no way to identify it. The principle might seem at first glance to be meaningless. It can be stated, however, in terms of the functions by which *I-V* is defined; when so stated it becomes open to verification.

I shall state the constancy principle in the form of three propositions. Each one states a necessary condition for predicting an unknown function. In so doing it raises two questions: (*a*)

[3] This example deals only with isolated variables. Wherever two or more *S-V*'s converge on *I-V*, there is a possible interaction. Counting each interaction as a separate measurement, we must add three first-order and one second-order interactions, bringing the total number of determinations to nine. Without *I-V* the total would be 14. Actually the added labor of measuring interactions will vary with the efficiency of experimental designs.

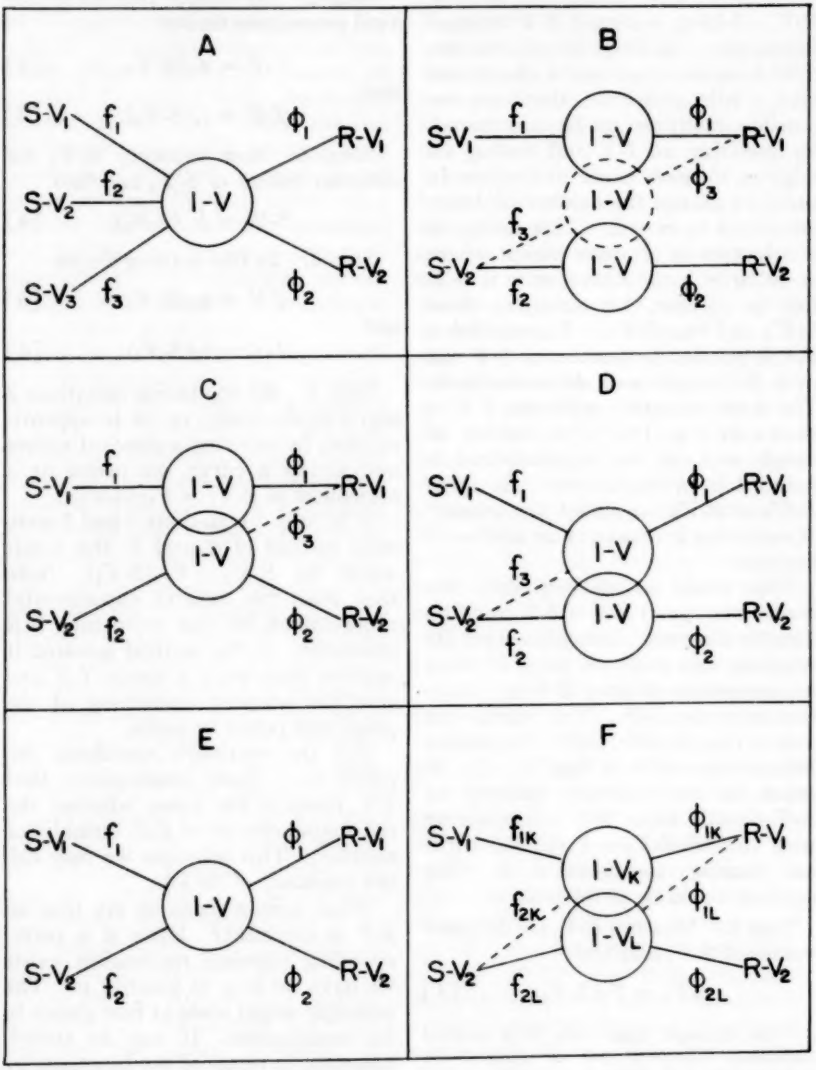


FIG. 1. A. Relations of intervening variable (*I-V*) to stimulus variables (*S-V*) and response variables (*R-V*). Functions relating *I-V* to *S-V* denoted by f; *I-V* to *R-V*, by ϕ. B. Hypothetical situation requiring constancy of *I-V*. Solid lines = known functions; broken lines = functions needed for prediction. C. Same situation with *I-V* constancy as defined by proposition I. D. *I-V* constancy by proposition II. E. *I-V* constancy by both propositions I and II. F. Hypothetical situations *K* and *L* requiring *I-V* constancy by proposition III.

whether the condition is in fact necessary; or (*b*) whether the condition is, in any particular case, met. As to question *a*, I shall be satisfied to answer it by "intuitive" argument rather than rigorous proof, so that we may proceed at once to empirical evidence bearing on question *b*. If I say that a proposition "holds" or is "valid," I shall mean that the answer to question *b* is "yes."

Proposition I: Given a single situation in which the following functions are known: $f_1(S\text{-}V_1) = I\text{-}V = \phi_1(R\text{-}V_1)$ and $f_2(S\text{-}V_2) = I\text{-}V = \phi_2(R\text{-}V_2)$. A necessary condition for predicting $R\text{-}V_1 = F_{12}(S\text{-}V_2)$ is that $I\text{-}V = f(S\text{-}V_2)$ is the same whether derived from $R\text{-}V_2$ or $R\text{-}V_1$.

Figure 1B illustrates the problem. Solid lines represent the situation given; the broken line represents the prediction to be made. Proposition I states that this prediction can be made only if $f_2 = f_3$; i.e., if the situation can be depicted as in Fig. 1C. Obviously if $f_2 \neq f_3$ equation 6 above could not be used in Step 5 to derive F_{12}.

But proposition I, though necessary, is not sufficient for a solution of the problem, since equation 2 is still of uncertain validity. The second requirement may be stated as follows:

Proposition II: Given the problem stated in proposition I, a necessary condition for predicting $R\text{-}V_1 = F_{12}(S\text{-}V_2)$ is that $I\text{-}V = \phi(R\text{-}V_1)$ is the same whether generated by $S\text{-}V_1$ or $S\text{-}V_2$. Starting with Fig. 1B, proposition II requires that $\phi_1 = \phi_3$, a condition which would validate equation 2 for use in Step 5 and would change the diagram to the one shown in Fig. 1D. Finally, if the conditions of both propositions are met, Fig. 1E shows the situation and demonstrates graphically the constancy of *I-V* for the two pairs of *S-R* variables.

Propositions I and II, taken together and generalized to any number of variables, define *I-V* constancy for a single situation. It would be of enormous advantage, both to the builder and to the user of theory, if the principle could be extended to cover situations differing in some respects. A function derived from an experiment on eyelid conditioning, for example, might then be combined with data on conditioned finger withdrawal to provide a new equation applicable to both. Such an assumption is implicit in several of Hull's derivations (e.g., Postulates II, V, VI, XVIA [16]). A more or less formal statement of it follows:

Proposition III: Given two situations, *K* and *L*, in which the following functions are known: in *K*, $f_1(S\text{-}V_1) = I\text{-}V = \phi_1(R\text{-}V_1)$; in *L*, $f_2(S\text{-}V_2) = I\text{-}V = \phi_2(R\text{-}V_2)$. A necessary condition for making cross predictions in either situation is that the corresponding functions of *I-V* are the same in both.

For example, suppose we wish to predict $R\text{-}V_1 = F_{12}(S\text{-}V_2)$. (Propositions I and II are, of course, assumed to hold for *K* and *L* separately.) Figure 1F contains the essentials. Proposition III states that the desired function can be derived only if $\phi_{1K} = \phi_{1L}$ and $f_{2K} = f_{2L}$. As in the previous cases this condition is implied by Step 5 of our derivation procedure; it implies in turn that $I\text{-}V_K$ and $I\text{-}V_L$ should be drawn to coincide.

It follows from this discussion that an *I-V* defined in terms of several *S* and *R* variables can be a powerful tool for making predictions or for deriving new functions. Indeed, the method here outlined may turn out to be an essential part of any theory that makes prediction possible. Its success, however, is clearly contingent on its ability to meet a set of exacting requirements; viz., those laid down in the constancy principle.

HULL'S ${}_sE_R$ ON TRIAL

Until recently no psychological theory was far enough advanced to make such tests possible. During the past decade, however, Hull and his students (**7, 9, 17**) made a concerted effort in this direction. By applying Thurstone's scaling technique to the bar-pressing latencies (${}_st_r$) of rats in successive reinforced trials (N), they were able to define the central construct of his system, reaction potential (${}_sE_R$), as a precise mathematical function of N on the one hand and ${}_st_r$ on the other. These equations were then used, much as in the steps outlined above, to derive other postulates. At the same time Hull provided a means of testing the predictive power of his system insofar as it hinges on the constancy of ${}_sE_R$.

In this section I shall consider evidence on how well ${}_sE_R$ meets the criteria of constancy stated in propositions I to III, and shall take them up in the order III, I, and II.

Evidence on proposition III. Hull's treatment of bar-pressing latencies as a function of number of reinforcements ($\mathring{N} = N - 1$) led to the following equation (approximately):

$$ {}_sE_R = 3.1(1 - 10^{-.03\mathring{N}}). \quad [7] $$

Zeaman (**36**) applied the same scaling procedure to starting latencies and running times in a Graham-Gagné apparatus[4] with results as follows:

$$ {}_sE_R(\text{lat.}) = 3.6(1 - 10^{-.09\mathring{N}}), \quad [8] $$

$$ {}_sE_R(\text{run.}) = 4.7(1 - 10^{-.07\mathring{N}}). \quad [9] $$

Seeking to explain the difference in exponents between equations 7 and 8, Zeaman suggested the much larger reward used in his experiment as the most likely factor. But both his own latency data, as he pointed out, and Crespi's (**5**) running time reciprocals as treated by Hull (**15**) showed that amount of reward affected asymptotes rather than exponents. Zeaman's argument implies that the function relating ${}_sE_R$ to ${}_st_r$ varies with amount of reward, a possibility which, in the light of other evidence to be presented below, should certainly be checked.

Comparing equations 8 and 9, Zeaman attributed the lower asymptote of the latency curve to a longer delay of reinforcement. But this factor should raise the asymptote of equation 7 still higher, since the Yale rats received reinforcement immediately. Rather than attempt to resolve these discrepancies *ad hoc*, we should probably admit that bar-pressing latencies and runway latencies yield I-V functions with different constants, then launch a systematic search for the factors responsible.

Greater consistency might well be expected between two sets of bar-pressing data. Yamaguchi (**33**) conducted a study in Hull's laboratory using apparatus practically identical with that of Felsinger *et al.* (**7**). He, too, applied Thurstone's method to his latencies and fitted a curve to the plot of ${}_sE_R$ values against successive reinforcements. His equation, to be compared with equation 7, was approximately:

$$ {}_sE_R = 7.6[1 - (\mathring{N} + 1)^{-.2}]. \quad [10] $$

Contrary to expectation, the two equations differ not only in constants but in type. Comparing the experiments we find so many changed conditions that we can only guess which ones are responsible. Differences in maintenance schedule, pretraining procedures, and distribution of trials are equally likely candidates.

Further evidence in the same direction may be found in Hull's Postulate

[4] The rats traversed a 3-ft. elevated path from a starting box to a goal box containing food.

XVI, parts A and B. There we find two equations for ${}_sE_R$ as a function of unreinforced trials to extinction (n); viz. (approximately),

A. ${}_sE_R = 4.0(1 - 10^{-.01n})$, [11]

B. ${}_sE_R = .12 \times 10^{.06n} + 2.1$. [12]

Equation 11 was derived indirectly by using a variant of equation 7 in conjunction with bar-pressing data from earlier experiments by Perin (**24**) and Williams (**32**). Equation 12 was derived, also indirectly, by Yamaguchi (**33**), who combined equation 10 with extinction data from his own experiment. It will be noted that equation 11 describes a negatively accelerated, and equation 12 a positively accelerated, curve. Hull's singling out of massed versus "quasi-distributed" reinforcements as the differential condition may or may not be correct. The point that concerns us is that reaction potential as measured turns out to be an exceedingly sensitive instrument; the conditions of its use must therefore be rigidly specified.

Evidence on proposition I. Here we turn from the comparison of two situations, involving unspecified S and R differences, to a comparison of different measures of the same response in the same situation, such as latency, relative frequency, intensity, or resistance to extinction. A difficulty in testing proposition I is to change R-V without also changing the response. Zeaman's study of latencies and running times was not crucial, since he was able to show that the two equations he derived for ${}_sE_R$ belonged to two different responses. A recent study by Logan (**21**), however, claims our attention.

Using response times and choice frequencies of two bar-touching responses with different delays of reward, Logan derived three measures of "differential excitatory tendency." His first measure, based on response speed, was a linear function of the difference between latency reciprocals to the short-delay and long-delay bars on single-presentation trials. His second measure, based on "preferential latency" in the same trials, was "the abscissa of the normal probability integral . . . at which the ordinate equals the proportion of times the response speed to the short delay bar is greater than that to the long delay bar" (**21**, p. 302) multiplied by $\sigma\sqrt{2}$.[5] His third measure was derived in the same way from the percentage of choices of the short-delay bar on joint-presentation trials. Instead of plotting all three measures against number of reinforcements and comparing the curves of best fit, Logan tested the inference that the relations among them should be linear.

Logan's analysis would have been more decisive if he had scaled his first measure by Hull's adaptation of Thurstone's method instead of averaging latency reciprocals. The fact that the two estimates based on response speeds and preferential latencies gave a straight-line relationship implies that the former was indeed equivalent to ${}_sE_R$, but the matter needs checking to avoid circularity. His second plot, of response speed against percentage of choice measures, also showed linear trends. Departing from theory, however, these trends had steeper slopes than the first one and differed from one part of the experiment to another. Logan also showed that "although the animals developed a 100 per cent choice of the short delay bar, they were responding more rapidly to this bar on only about 90 per cent of the compared latencies" (**21**, p. 306).

Again the question arises, do these

[5] σ is roughly equivalent to the standard error of oscillation of ${}_sE_R$ defined by Hull in Postulate XII (**16**).

measures refer to the same response? Measures 1 and 2 obviously do, since they represent different ways of treating the same numbers. But is a response to one of two bars presented together the same—i.e., determined by the same external and internal conditions—as a response to the same bar presented alone? The answer must await the verdict of a growing body of experiments comparing discrimination between stimuli simultaneously and successively presented (**1, 10, 20, 25**).

Logan himself appealed to stimulus patterning, itself the product of learning, to account for the divergence of his free-choice and forced-response measures. Another factor may be partly responsible. Theoretically $_sE_R$ to each bar fluctuates from moment to moment in a roughly normal distribution with a constant standard error of oscillation (σ_O). On the assumption that the two distributions of these momentary values ($_sE_R$) are independent, their differences are distributed with $\sigma_D = \sigma_O\sqrt{2}$. The percentage of times the response to one bar is faster than to the other is determined by the ratio of the mean difference to its standard error, which can thus be read from a table of the normal probability integral.

Each single-bar response may be held to represent one $_sE_R$ value, but a choice between two bars may represent more than one $_sE_R$ to each. A rat frequently hesitates, looking from one to the other before he chooses. Such "vicarious trial and error (VTE)" (**23**) samples a number of $_sE_R$'s. It would be plausible, therefore, to say that a single trial provides a difference between the means of two samples and that percentage of choice is determined by the distribution of these differences between means. Letting n stand for the mean size of a sample, we may approximate the standard error of this distribution by $\sigma_{\overline{D}} = \sigma_D/\sqrt{n}$. If, for example, we assume a mean of three VTE's per trial, we should use $\sigma_{\overline{D}} = \sigma_O\sqrt{2/3}$ instead of $\sigma_D = \sigma_O\sqrt{2}$ as on forced trials. It follows that a given differential $_sE_R$ would theoretically produce a larger percentage of choices of the short-delay bar than of faster responses to it, which is what Logan found.

This hypothesis can be checked. In an experiment comparing successive with simultaneous discrimination any condition that increases VTE on the joint-presentation trials should increase the discrepancy between preferential latency and percentage of choice. Tolman (**31**) has suggested a number of conditions that might be varied for this purpose, such as enforced delay at the choice point, difficulty of the discrimination, and the use of normal as compared with brain-injured rats.

As to the constancy of $_sE_R$ in the sense of proposition I, Logan's study confirms it, or at least offers no evidence against it.

Evidence on proposition II. A difficulty in making a direct test of proposition II is that it demands the use of Thurstone's method in connection with two *S-V*'s, whereas so far no study has applied it to more than one. To obtain an expression relating $_sE_R$ to $S\text{-}V_2$ we must, therefore, proceed indirectly, using a second *R-V* and tacitly assuming propositon I. This means that we are not testing proposition II alone but I or II or both. Two sets of data are available for making such indirect tests, one provided by Yamaguchi (**33, 34**), the other by Perin (**24**). A third study, by Kimble (**19**), gave latencies under conditions comparable with Perin's; they will therefore be used to supplement Perin's data.

Figure 2 is presented to illustrate the test. Here the general symbols

TABLE 1

EMPIRICAL AND THEORETICAL FUNCTIONS TAKEN FROM YAMAGUCHI (34) TO TEST PROPOSITION II

Steps	Empirical	Theoretical
1	(a) ${}_sI_r = F_1(N)$	(b) ${}_sE_R = \phi_1({}_sI_r)$ (c) ${}_sE_R = f_1(N)$
2	(d) $n = F_{21}(N)$	(e) ${}_sE_R = \phi_2(n)$
3	(f) $n = F_2(h)$	(g) ${}_sE_R = f_2(h)$

Note.—For explanation of symbols see Fig. 1 and 2.

of Fig. 1E are replaced by specific ones standing for the variables used in the two experiments. The hypothesis tested is that ϕ_1 is constant whether derived by varying N or h, the number of hours of food deprivation.

Yamaguchi's purpose was to find an equation for drive (D) as a function of h. His method, similar to the one outlined above, consisted of a series of steps the essentials of which are summarized in Table 1. Step 1 was to measure bar-pressing latencies in 88 reinforced trials, derive Thurstone ${}_sE_R$'s, and fit his (Yamaguchi's) equations b and c, relating them to ${}_sI_r$ and N. Equation c appeared as equation 10 above. Step 2 was to extinguish the response in several groups after different numbers of reinforced trials and, by substituting ${}_sE_R$ values from equation c, fit equation e for ${}_sE_R$ as a function of n (see equation 12 above). Note that at this point the soundness of proposition I was implied. In Step 3 the response was trained and extinguished after different amounts of food deprivation. These data, combined with equation e, led to equation g for ${}_sE_R$ as a function of h. By assuming equivalence of ${}_sE_R$ and D at the limit of practice, Yamaguchi finally emerged with the equation for $D = f(h)$ that appears in Hull's system as Postulate V.

It should now be possible to combine equations b and g and predict the values of ${}_sI_r$ to be expected after differing amounts of h. If proposition II holds, and if proposition I can be assumed, the predicted latencies should agree with those actually obtained. Fortunately Yamaguchi published median latencies for trials 72 and 88 of training in order to show that ${}_sH_R$ had reached its asymptote. These data, averaged for the two trials, may be compared with the theoretical latencies in Table 2. Whereas the calculated values, based on the variation of ${}_sI_r$ (and n) with N, drop rapidly, especially between 12 and 48 hours, the empirical measures show no trend at all.

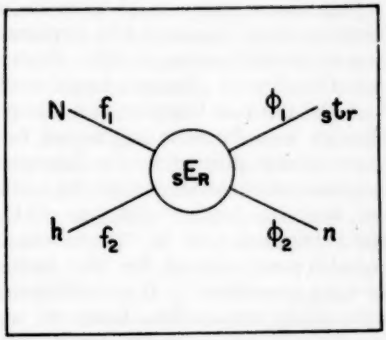


FIG. 2. Experimental situation for testing proposition II provided by Yamaguchi (34) and Perin (24). N = number of reinforcements; h = hours of food deprivation; ${}_sE_R$ = reaction potential; ${}_sI_r$ = latency; n = number of unreinforced trials to extinction.

TABLE 2

MEDIAN BAR-PRESSING LATENCIES (SECONDS) AFTER DIFFERENT DEPRIVATIONS (h) AS CALCULATED USING ${}_sE_R$ AND AS OBTAINED BY YAMAGUCHI (34)

h	Calculated	Obtained
3	3.56	1.00
12	3.16	1.11
24	0.89	0.93
48	0.16	0.98
72	0.19	1.01

This discrepancy could have been foreseen from Yamaguchi's explanation of his indirect approach. Direct quantification of ${}_sE_R$ as a function of h at the limit of learning was "inapplicable here because significant between-group differences in latencies were not obtained except on the earliest training trials" (**34**, p. 114). He attributed this to "the unusual enfeeblement caused by the maintenance procedure." If enfeeblement were solely responsible, however, we should not expect resistance to extinction to vary with hunger as it did. The equalizing of latencies of deprivation groups in the course of training suggests two things: (*a*) As ${}_sE_R$ increased it approached a ceiling imposed by physical conditions limiting speed of response. (*b*) If latencies were measured after fewer training trials they might agree more closely with prediction.

With respect to the first implication Cotton (**4**) has reported running times in an 8-ft. runway after 440 reinforced trials, 110 under each of four deprivation periods from 0 to 22 hours. When all test trials were included, clear-cut trends between h values appeared, but when trials with irrelevant responses were excluded, the range of group means for the four drive levels was only about a half second. Although this variation was statistically significant, the author concluded that "the major effect of increased deprivation is to reduce the occurrence of competing responses rather than to increase speed of running per se" (**4**, p. 196). Since bar pressing allows less time for competing responses than running, Yamaguchi's negative finding is understandable.

As to the second implication, the experiments of Perin (**24**) and Kimble (**19**) provide a crude test. Perin gave his rats 16 reinforcements of a bar-pressing response under 23 hours of deprivation, then extinguished four groups at different deprivation periods. Kimble trained his animals, hungry for 24 hours, to push a panel; after 15 reinforced trials he gave tests under different amounts of deprivation in random order. Perin combined his results with the earlier findings of Williams (**32**) in order to state resistance to extinction as a joint function of N and h. By assuming constancy it is possible to write component functions in terms of ${}_sE_R$. Since Perin incidentally reported a few latencies and Kimble made them his primary measure, we may again test the constancy assumption.

Our procedure is practically identical with Yamaguchi's and conforms to Table 1 except for the source of the functions. Steps 1 and 2 had already been taken by Hull (**15**, p. 110) in deriving Postulate XVI A, as described above. Equations a, b, and c came from a later experiment (**7**) (see equation 7 above), equation d represented the Perin-Williams data, and equation e emerged as equation 11.[6] Step 3 consisted of using this equation together with Perin's data (equation f) to find ${}_sE_R$ values corresponding to his degrees of deprivation (equation g). To do the same for Kimble's data it was necessary first to compute n values for his deprivation periods by equation f (Perin's equation 4 [**24**]). Finally, again invoking proposition III, I used equation b[7] to change both sets of ${}_sE_R$'s into theoretical latencies.

[6] Propositions I and III seem especially vulnerable here, since equation c was based on one trial a day, while Perin's and Williams' training was massed.

[7] To make equation b consistent with the other equations, I used ${}_sE_R = 2.845(\mu_r)^{-.482} - .173$, obtained by adding .426 to Hull's equation 9 (**9**, p. 518) in order to measure from absolute zero.

TABLE 3

MEDIAN LATENCIES (SECONDS) AFTER DIFFERENT DEPRIVATIONS (h) AS CALCULATED USING ${}_sE_R$ AND (AFTER CORRECTION) AS OBTAINED BY PERIN (24) AND KIMBLE (19)

h	Calculated	Obtained*	
		Perin	Kimble
1	6.04	3.82	
2	5.55		3.02
3	5.20	3.62	
8	3.38		2.55
15	1.93		1.55
16	1.76	1.81	
23	1.08	1.00	
24	0.93		1.00

* Perin's scores were divided by 15, Kimble's were multiplied by 5.

The results for all h values used in the two experiments are presented in columns 1 and 2 of Table 3.

The actual latencies differed so widely from one study to another that correction factors had to be employed. One of Perin's and one of Kimble's groups, tested after 16 and 15 trials, respectively, gave median latencies of 15 and 0.2 seconds, compared with 0.95 second at the same stage of training in the standardizing experiment (7). I therefore divided Perin's medians by 15 and multiplied Kimble's by 5.[8] The corrected scores appear in columns 3 and 4 of Table 3.

Here the empirical measures, unlike those of Table 1, show a decided slope, although still not so steep as the derived ones. We are not entitled, however, to attribute this trend to the earlier stage of training represented, since both Perin and Kimble trained and tested their animals under conditions shown by Yamaguchi (35) to produce drive stimulus intensity generalization. For this reason, as well as the circuitous nature of the derivation and the large arbitrary correction factors required, our comparison does little more than point up the need of a more adequate test.[9]

It is worth noting that our inquiry into proposition II challenges a basic assumption in Hull's system: that ${}_sH_R$, as defined in Postulate IV (16), is independent of the strength of drive under which it is acquired. The evidence for this assumption is mainly of two kinds: (*a*) Several investigators (18, 28, 29) have found resistance to extinction unrelated to hours of deprivation during training. (*b*) Perin fitted equations to his own and Williams' data on resistance to extinction as a function of number of reinforcements; he found the same exponential constants for training after 3 and 24 hours of deprivation. But Hull quantified ${}_sH_R$[10] on the basis of reaction latencies with successive reinforcements at a 24-hour drive level. To say that ${}_sH_R$, so quantified, is independent of drive during training does not follow from the evidence on resistance to extinction.

One reason why ${}_st_r$ may differ from n in this respect was suggested earlier: inertia, or perhaps a lack of competing responses, may keep ${}_st_r$ from reflecting the growth of ${}_sH_R$ beyond a certain limit. At high drive levels this ceiling may be reached earlier in training than at lower ones, resulting in a steeper and more sharply bent curve. The more pronounced inflection of equa-

[8] I estimated Kimble's latencies from his published graph (19).

[9] In developing the functional relationships of ${}_sE_R$ the Hull group (9) converted Perin's corrected latencies into ${}_sE_R$'s to plot against h. Their corrections involved an error in that they treated Perin's scores as seconds, whereas he actually reported them in minutes. In view of the weaknesses just mentioned, as well as the probable contamination of Perin's data by extinction, a more accurate treatment is not attempted here.

[10] Since ${}_sE_R$ is treated as a linear function of ${}_sH_R$, the same quantification applies to both.

tion 10 as compared with equation 7, for example, may well indicate a stronger drive due to the severe maintenance schedule in Yamaguchi's experiment. But what is obviously required is an independent investigation parallel with the original study (7) to determine ${}_sE_R$ as a function of N under, say, three hours of deprivation. If the foregoing argument is sound, the resulting function should have a smaller exponential constant than the one in Postulate IV.

It may be objected that the argument, even if sound, is trivial, since the underlying construct may be independent of drive even though its distorted image is not. Such an objection loses sight of the *sine qua non* of a scientific theory, the operational definition of its constructs. If ${}_sH_R$ as derived from latencies is found to be dependent on h, the only way its independence can be saved is to redefine it in terms of other operations.

SUSPENDED SENTENCE

To summarize, I have tried to evaluate the usefulness of intervening variables in the explanation, or prediction, of behavior. I argued that the usefulness of an *I-V* depends on the constancy of its relations to *S-V*'s and *R-V*'s as specified in three propositions. I then examined available evidence in order to test the constancy of one *I-V*, Hull's ${}_sE_R$, in the only system sufficiently quantitative for such tests to be applied.

What is the outcome? It would obviously be premature to pass judgment on the viability of Hull's construct. Our inquiry merely demonstrates the kind of validation a theory must undergo if it is to make itself useful. This requirement carries several implications for method in theory construction which may be worth stating. They will be couched in terms of animal experimentation for illustrative purposes only.

1. Stability of functional relationships in behavior research demands the utmost uniformity in operations of measurement. While paying lip service to this rule, rat experimenters seldom observe it. As a result, if two findings from different laboratories, or even by different investigators in the same laboratory, confirm each other at all, they can seldom be described by the same equation. A systematic structure can emerge only from rigorously standardized conditions. This means, first, concentration of effort on a few standard situations: enclosed runway, elevated path, Skinner box, single **T** maze, discrimination box, or jump stand. But more than that, it means agreement on a set of uniform conditions for each situation; e.g., length of runway, work required to press bar, maintenance schedule, pretraining procedures, distribution of trials, age, sex, and strain of subjects.

2. In each situation functional relations are established between several *R-V*'s (e.g., latency, relative frequency, intensity, number of trials to extinction) and several *S-V*'s (e.g., number of reinforcements, hours of deprivation, amount of reward, delay of reward, stimulus intensity).

3. One or more theoretical constructs, or *I-V*'s, are conjectured to mediate these relations. To give such an *I-V* predictive power it must be possible to quantify its relation to a single *S-V* independently on the basis of more than one *R-V*. So far ${}_sE_R$ as a function of N has been derived only from latencies and, by way of informal demonstration (14), from response frequencies.

4. Similarly, an *I-V* measured by means of a single *R-V* must be independently determined as a function of

more than one *S-V*. So far this has not been done, although it was included in Hull's program (**14**).

If the undertaking is to succeed, certain uniform relations will appear within each situation and between situations. If they do not, new constructs and new scaling devices must be invented and tried.

Advance from simple to complex behavior is implicit in the method outlined. Functions established in runway and Skinner box are tried out with data from **T** maze and jump stand, from experiments on successive and simultaneous discrimination. Here, too, Hull has led the way with his analysis of simple and compound trial-and-error learning (**16**).

I do not claim this is the way the science of behavior is actually developing. There are those who would deny that it is even an ideal method, holding, with Harlow (**11**), that simple things may be used to explain simpler things but seldom more complex ones. Two issues are likely to be confused here. One, which motivated the present inquiry, lies between positivists and systematists; the other divides rat psychologists and human, or primate, psychologists. Although it happens that the most advanced behavior system to date is closely bound up with experiments on rats, the connection is not necessary. A theorist can conceivably build a psychological system without reference to rat behavior. But whatever his phylogenetic level, if his system is to have any considerable explanatory power he must shape it to meet the kinds of tests here proposed.

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PLEASURE-SEEKING BEHAVIOR AND THE DRIVE-REDUCTION HYPOTHESIS [1]

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During recent years, it has become increasingly apparent that notoriety, if not fame and fortune, can be achieved by repeatedly and vociferously attacking the drive-reduction hypothesis of reinforcement. Just why this hypothesis should have been singled out for special attention is not entirely clear. Perhaps it is indeed strikingly inferior to other conceptions. Perhaps it is one of the few whose formulation is specific enough to make attacks feasible. Or perhaps the onslaughts are manifestations of reaction formation by those whose drives have been reduced through the writing of other polemics. In any event, it must be admitted that these crusades have had a salutary influence. The defenders of the faith have been forced to think more clearly, to avoid circularities of definition, and to perform relevant supporting experiments.

Since these criticisms appear rather frequently in the pages of our psychological journals and books, there is always the danger that such half-truths as they may contain will mislead the casual reader, and that any residual virtues of the drive-reduction assumption will be needlessly obscured or even forgotten. The purpose of this paper, therefore, is to re-examine some of these contentions and to re-emphasize some relatively neglected facts and theoretically plausible mechanisms that bear importantly on the issue. No attempt will be made, however, to defend the notion that drive reduction is, in addition to stimulus-response contiguity, a necessary mechanism for all learning. The concern of this discussion is not with the drive-reduction hypothesis of learning per se. Instead, attention is focussed on the problem of whether behavior commonly described as "pleasure seeking" or "stimulation seeking" constitutes a genuine exception to the drive-reduction interpretation, and whether, without undue strain, such behavior can be coordinated with that conception.

Surveys of the history of the drive-reduction view (22, 28) show that the arguments of its opponents have been not only vigorous, but well diversified. Some writers, placing their trust in the principle of parsimony, have insisted that stimulus-response contiguity alone is sufficient to account for the growth of associative bonds, additional factors of decreased drive or tension being unnecessary. Others have held that the effects attending a consummatory response cannot act backward in time to affect the strength of a connection, or perhaps that "pleasant" consequences alone can produce learning, or even that learning can take place in the absence of both motivation and motivation reduction.

The criticism most frequently raised of late, and the one we shall be concerned with here, has been phrased in a variety of different ways. In all of them, however, a central role has been assigned to the observation that *a response may be learned or maintained even though marked increases in stimulation occur while it is in progress or even after its termination.*

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In one of the variations on this theme, emphasis is placed on the fact that organisms sometimes exhibit behavior that takes them from an initial state of relative quiescence into one marked by heightened activity and strong stimulation (13). The individual who deserts the quiet comfort of his easy chair for a lurching, noisy ride on a roller coaster is regarded as epitomizing this behavior. He is described as seeking strong stimulation. Since the increased stimulation follows closely upon the response of getting into the roller coaster, and since that increase is sometimes equated with a rise in drive, a problem is said to be raised for the drive-reduction view. In essence, this is the problem of an organism's exhibiting behavior resulting in so-called pleasurable or positive stimulation. It is the problem of why, if the drive-reduction view is correct, men and animals ever behave so as to obtain more stimulation rather than less.

The same basic criticism is sometimes expressed as a dissatisfaction with the explanatory use by drive-reduction theorists of the concept of secondary reinforcement. Hilgard (9), for example, points out that the phenomenon of secondary reinforcement is normally marked by an increase rather than a decrease in stimulation. If stimulus reduction is identified with drive reduction, then a rise in stimulation implies an increase in drive. As a consequence, appeals to secondary reinforcement to explain learning are said to lead to a weakening of the generality of the drive-reduction interpretation.

Still a third formulation of this objection arises from the finding that monkeys, when confined in an opaque-walled box, can learn manual responses for which no reward is given other than that of being allowed to look out of a window of the box (1). According to one interpretation of this result (7), the stimulation provided by the laboratory room beyond the box provides, or arouses, a drive. Consequently, if responses are actually learned when followed by such a drive increase, their acquisition cannot be ascribed to drive reduction. In short, if a response is followed by an increase in stimulation, irrespective of what happens then or later, and regardless of previous experiences with such stimulation, the drive-reduction concept is declared helpless to account for the learning of that response.

A fourth and final variation of this anti-drive-reduction motif rests on the supposed fact that organisms persist for unreasonably long periods in "behavior which is clearly punishing, or, at least more punishing than rewarding" (22, p. 546). The hypothetical author who is described as writing for a lifetime even though the completion of each opus is punished by an editor's rejection slip, and the masochist, are popular examples cited to support this notion. Though the cataloguing of such reactions as "more punishing than rewarding," is obviously unreliable, since hidden rewards may be operating, and "unreasonable persistence" is also difficult to assess, the belief in the existence of such persistent maladaptive behavior is apparently widespread.

To recapitulate, the drive-reduction assumption has been declared deficient for the following reasons: (*a*) Organisms seek out and approach situations or perform actions involving an increase rather than a decrease in stimulation. (*b*) The drive-reduction theorist is inconsistent if he tries to explain the learning of some responses by invoking the principle of secondary reinforcement with its attendant stimulation increments. (*c*) On the assumption that visual stimulation is drive arousing in monkeys, the acquisition of responses "rewarded" by such stimulation is evidence against the drive-reduction concept. (*d*) Organisms are supposed to

persist unreasonably in behavior regarded by observers as more punishing than rewarding. In all of these criticisms, stress is laid on the fact that *when increases in stimulation occur after a response has been initiated or after it ceases, the tendency to perform that response is not always weakened, and may even become stronger.* This failure of increased stimulation to function as a kind of "negative reinforcer" is proposed as a major stumbling block for the drive-reduction interpretation.

As an initial step in attempting to answer these criticisms, it may be noted that the proponents of the above views, in order to stimulate thought and present the best possible case for their position, have often emphasized rather bizarre and extreme examples. But the problem raised by these examples is not confined to the bizarre and the abnormal. The phenomena are typical of the simplest and most conventional kinds of behavior. One need not turn to the masochist for pertinent data; they abound in nearly all reactions. Though this point seems to have escaped notice, *almost every overt response, and many a covert one, is accompanied by an increase in stimulation.* In most instances, the level of stimulation provided by proprioceptive feedback and by fluctuating external cues must be higher during the response than either before or after. When a rat is first placed in a maze, it may not move at all for a while. When it does move, however, its reactions are likely to provide an increase in the level of both internal and external stimulation. But if stimulation is, in general, more intense at a later phase of the response than at the moment of its initiation, a problem arises as to whether this constitutes a rise in drive and hence an embarrassment for the drive reductionist. Clearly, this is the same problem that lies at the core of the criticisms that have been mentioned, and just as clearly, it too requires a carefully considered analysis.

Granting, then, that a great many responses, though probably not all, are attended by heightened stimulation, by what stratagems can these stimulus increments be coordinated with the suppositions of the drive-reduction view? One alternative might be to assume that *the sensory consequences of most responses are practically never intense enough to provide increments to the drive level.* But this assumption seems unsatisfactory for the following reasons: (*a*) It implies that when stimuli really are strong, a drive results, an implication that reintroduces the problem to be solved. (*b*) It tacitly assumes that when stimuli do produce a drive the response should be abandoned or at least not learned. (*c*) It raises the further problem of how to specify the precise level of intensity at which stimuli become or induce drives.

A second avenue of escape from this dilemma has been opened by one of Miller and Dollard's proposals (15). They suggest that *heightened stimulation following a response is not incompatible with the drive-reduction position because such stimulation may lower the total stimulation to a level below that prevailing initially.* To use their example, an individual lost in a forest at night does get more stimulation when he sees a light in a cabin window. But seeing the light may actually decrease the totality of stimulation by eliminating internal excitations arising from anxiety or apprehension. Although this solution has substantial merit, the range of cases to which it applies appears to be relatively limited. Moreover, since the rise in stimulation is closer to the response than the decline, a problem remains as to why the decline should override the "punishing" effect of the more immediate rise.

A third possibility, the one to be defended here, is simply that *an increase*

in stimulation, even when relatively intense, need not always be categorized as an increase in drive. That is, it is sometimes profitable to treat strong stimuli as drives in our formulations for predicting behavior, and sometimes not. The adoption of this view makes necessary the abandonment of the position that each and every stimulus necessarily has drive properties simply because it is physically intense. It means that the definition of a drive must include more than measurements of, or inferences about, the sheer physical strength of a stimulus. It means that a given stimulus may have drive value for some individuals, but not for others; or for one individual at one time, but not at another. This is apparently essentially the same point that Skinner (23) makes when he asserts that a drive is not a stimulus.

With respect to the problem under consideration, this suggestion, that strong stimuli need not always engender drive, has numerous implications. Generally speaking, it makes imperative a careful re-examination of all instances in which strong stimulation is supposed to present a paradox for the drive-reduction view. Specifically, it leads to the conclusion that whenever the jostling and noise of the roller coaster can be disqualified as drive increments, the occurrence of the behavior does not constitute an anomalous example. The masochist who beats himself with a hammer may not be providing sensory excitation whose role as a behavior determinant is covariant with drive. In all these instances, the increase in physical stimulation is present, but the presumed increase in drive, along with its paradoxical implications, vanishes.

Merely to assert, however, that strong stimuli do not always function as drives does not contribute much to the resolution of our difficulties. We need to know when, and under what conditions, stimuli will or will not have drive-like effects. We need to list specific factors other than stimulus intensity which must be taken into account if a maximally useful definition of stimulus-induced drive is to be achieved. Whether the resulting definition is, in fact, useful, will depend on whether certain agreed-upon behavioral consequences are found to accompany or to follow the presentation or removal of the stimulus. Consequences such as the intensification of behavior or the occurrence of learning might serve adequately in this capacity, though no firm decision need be made here.

In attempting to specify these additional factors, let us first consider those which lead to the conclusion that a given stimulus, though strong, will *not* operate as a drive.

One factor capable of altering the drive properties of a stimulus is the amount and kind of previous experience that the organism has had with that stimulus. Thus *a physically intense stimulus tends not to produce a drive if an organism has been exposed on successive occasions to gradually increasing values of that stimulus and if each presentation has been followed by some kind of drive reduction.* Some of the most convincing evidence to support this contention comes from the conditioning studies of Pavlov. He found that stimuli such as strong electric shocks, wounding of the skin severe enough to draw blood, and cauterization of the skin lost their noxious aspects if, when used as conditioned stimuli for an alimentary response, their intensity was gradually increased. The process of neutralization was apparently quite complete, since according to Pavlov's report ". . . not even the tiniest and most subtle objective phenomenon usually exhibited by animals under the influence of strong injurious stimuli can be observed in these dogs. No appreciable changes in the pulse or in the respiration occur in these animals . . ."

(20, p. 30). Incidentally, the fact that the dogs in these experiments were fed for a few seconds after each stimulation appears to be especially significant. Prior to the conditioning training, each of the stimuli apparently had drive value. Marked alterations in respiration and pulse, as well as violent motor reactions directed toward removal of the stimuli or escape from them, were observed. That a complete tolerance was developed for these strong stimuli may be taken as evidence for their loss of drive-arousing capacity. According to Liddell (11), the major findings in these studies have been amply corroborated in studies at the Cornell Behavior Farm.

Further striking evidence to support the view that stimuli normally describable as painful or drive arousing can lose these attributes and become neutral or even "pleasant" if presented at progressively increasing strengths and followed by primary rewards is to be found in Masserman's studies of "experimental masochism." In these experiments, cats were trained to press a switch to obtain food. Air blasts of gradually increasing intensity were then administered each time the switch was depressed. Under these conditions, the air blast can be raised to an intensity that ". . . would be tolerated by no 'normal' cat and yet, because it is now part of the signal-complex leading to feeding, the experimental animal not only is not disturbed by it but, even in the absence of the food reward itself, frequently works the switch to experience the air blast as a subsidiary goal" (12, p. 57). That results of this kind are not restricted to animal subjects is apparent from Slutskaya's study (24) in which infants who were pricked with a needle and then fed came to exhibit anticipatory eating responses to the sight of the needle.

In addition to these experimental data, there are abundant anecdotal illustrations, which, though less well documented, also tend to support this contention. Straight whiskey, when first ingested, typically elicits rather violent defense reactions. Because of this, the novice drinker usually begins with sweet liqueurs, "pink ladies," and wines, and slowly works his way through a series of beverages characterized by the gradual disappearance of cola and ginger-ale additives. Finally, only plain water or even nothing need be mixed with the raw product. To the hardened drinker, straight whiskey does *not* taste bad—not bad at all! (It is thus that a product euphemistically labeled "neutral spirits" becomes indeed psychologically neutral.) That the ingestion of alcoholic beverages is often followed by the reduction of one or more drives is common knowledge. It seems needless, therefore, to speculate here as to which drive, when reduced, provides the principal support for this process of neutralization.

Closely parallel steps seem to be involved in acquiring a taste for highly peppered sauces and in learning to eat the kinds of cheeses that Henning would certainly have located at the putrid corner of his olfactory prism. In each instance samples of the commodity, diluted to the point of toleration, are presented initially and are followed by gradually strengthened versions until a terminal level, limited apparently only by the manufacturer's ingenuity, has been reached.

This process of "demotivating" a strong stimulus seems to have much in common with the phenomenon of *negative adaptation*, especially as it has been described by Guthrie (5). Though he would doubtless not regard the drive-reduction aspect as necessary, he might agree that the task of adapting the horse to the feel of saddle and rider by gradually increasing the weight on its back would be facilitated by the fre-

quent administration of lumps of sugar and crisp Washington apples.

Under some conditions, it might be predicted that the intense proprioceptive stimulation and muscular strain due to prolonged work should have drive-like effects. But if an organism gets appropriately reinforced training, it can acquire a tolerance for the stimulative effects of repetitive muscular effort that is little short of astounding. Rats and pigeons can be trained to make hundreds of responses for a single bite of food if the percentage of reinforcement is high initially, and if the reduction in frequency of reinforcement with further trials is sufficiently gradual. In such instances, apparently, the stimulation accruing from a multitude of successive reactions does not function as a drive, since behavior such as resting, though followed by the cessation of such stimulation, is not strengthened.

Having noted that, in certain learning situations, strong stimuli tend to lose their drive-arousing functions, we may now inquire as to what mechanisms may be involved in this process.

One such mechanism appears to be the transformation of a drive-arousing stimulus into a cue for specific responses appropriate to the situation. In effect, *a stimulus loses its power to act as a drive as it acquires strong associative connections to well-integrated, directed bits of behavior.* For the albino rat, a bright light has demonstrable drive properties. Responses instrumental in escaping from the light, or in turning it off, are readily acquired. But if the light is located near food, and if, through appropriate training, it becomes associated with reactions of approaching and eating, its original drive characteristics will be diminished.

This general notion, that the drive function of a powerful stimulus declines as it acquires new, behavior-directing properties is supported by several lines of evidence. Hebb, for example, makes essentially this point when he asserts that ". . . potentially painful stimulation is not disruptive (that is, not pain) when it forms part of a stimulation that has well-organized central effects" (8, p. 199). In studies of widely distributed periodic reinforcement, stimuli arising from muscular tension and fatigue tend, early in learning, to prevent further responding. But after learning is well advanced, they function as cues for additional reactions. The drive aspects of the strong olfactory and gustatory stimuli arising from whiskey disappear as those cues become associated closely with elbow-bending and swallowing responses. The increase in pleasant affect found by Peters (21) to accompany the making of positive responses to initially unpleasant stimuli may be a comparable phenomenon, and there are a number of parallels between his judgmental theory of feeling and the position taken here.

An unpublished experiment by Miller and Davis [2] illustrates nicely both the manner in which a strong stimulus can acquire associative properties and the importance of making the increases in its intensity gradual if the process of demotivation is to be successful. In their study, rats were first trained to traverse a 2-meter alley for food. Momentary shocks were then administered at a distinctively marked section in the middle of the alley. When shock intensity was strengthened gradually on successive trials, the animals continued to dash rapidly down to the end containing food. But when shock was increased suddenly, the phenomenon of continued running did not appear. Seemingly, when the shock was increased in small steps, it acquired a tendency to elicit running rather than stopping. By becoming associated with

[2] Miller, N. E., & Davis, M. The influence of the positions of reward and punishment in the response sequence. Personal communication, 1953.

the approaching response it relinquished its power to act as a drive. On the basis of this evidence, then, it would appear that drive-arousing stimuli tend to become neutralized as they become converted to cues having strong associations with well-integrated, ongoing responses.

A second mechanism of importance in effecting the neutralization of strong stimuli during learning involves the acquisition by such stimuli of secondary reinforcing power. The presumption here is *that stimuli lose their driving potentialities, in part at least, as they acquire the capacity to reinforce antedating responses.* The loud buzz produced by the food-delivery magazine in a Skinner box may at first be drive arousing and disturbing to the naive rat. But after being repeatedly paired with food it gains the capacity to reinforce completely new acts and its manifest driving aspect is reduced.

Keller and Schoenfeld (10) stress this mechanism in their analysis of human aversions. As has been done here, they note that originally unpleasant stimuli may lose this character and become acceptable, or even sought after, as a consequence of training. This modification of the properties of the stimulus they attribute not to the outweighing of the aversion by a conflicting drive, but to the aversive stimulus' becoming a secondary reinforcement. They apply this interpretation to masochism, as well as to laboratory studies in which electric shocks administered during reinforced bar-pressing trials acquire the capacity to produce a rapid acceleration in response rate during extinction.

Precisely what happens when a stimulus becomes transformed from a drive arouser to a secondary reinforcer is not clear. One interpretation, which is consistent with the drive-reduction view, would stress the kind of response that becomes associated with the stimulus when it acquires its reinforcing power. If that response is capable of interfering with, or reducing the drive condition of the moment, the net effect of the secondary cue will be an actual diminution in drive. Insofar as this holds true, it means that any inherent drive-arousing power of a secondarily reinforcing stimulus diminishes as it achieves the strength to elicit a drive-reducing reaction. An interpretation of this sort has been proposed for the special case of acquired drives by Miller and Dollard (15). For them, cues can act as learned rewards or secondary reinforcers when they acquire a tendency to inhibit or relax a response that is functioning as a drive. This possibility, that some kind of drive-reduction process underlies the phenomenon of secondary reinforcement, has also been expressed by Mowrer (16), Osgood (18), and Farber (3).

Evidence to support this view comes from diverse sources. Spragg (25), for example, has shown that the intense drive resulting from the withholding of drugs in addicted monkeys can be temporarily reduced by a hypodermic injection of saline solution. The sight and feel of the needle seem to elicit responses of relaxing that are incompatible with whatever states or reactions characterize the condition of abstinence. Wolff and Goodell (29) have found that the administration of a placebo falsely described as aspirin can produce an actual increase in a subject's threshhold for pain. And Gantt (4) has observed that anxiety symptoms in neurotic dogs are completely inhibited by sexual stimulation even prior to orgasm.

It thus appears that learning plays a significant part in the "demotivation" of strong stimuli by leading to the formation of close associations between those stimuli and ongoing activities and by building up the capacity of those stimuli to serve as reinforcers.

In addition to the role played by learning, there are at least two innate

processes capable of accomplishing the same result, at least temporarily. One of these is the familiar mechanism of *sensory adaptation*. Instances of this are common in the visual and olfactory modalities, but more striking ones are to be found in research on cutaneous pain. Stone and Jenkins (26), for instance, in a detailed review of such studies, report frequent confirmations of the phenomenon of adaptation to pain. In experiments employing needle algometers, complete pain adaptation was found in approximately 80 to 100 per cent of the instances in which it was studied.

The second innate mechanism may be described, for want of a better label, as *competitive stimulation*. This refers to the fact that the effective strength of pain-arousing stimuli can be weakened by the simultaneous presentation of other stimuli. Hardy and his collaborators (6) state that the pain threshold for one region of the body can be raised as much as 35 per cent by intense stimulation of some other region, a finding confirmed by Parsons and Goetzl (19). Hebb (8) also remarks that stimuli normally capable of provoking pain, and hence presumably drive, may fail to do so when combined with other sources of stimulation. Just how widespread the action of this mechanism may be is uncertain, but it is a common observation that pain can often be reduced by tensing the muscles or by gritting the teeth.

These then are some of the qualifying factors which, when incorporated into a definition of stimulus-induced drive, lead to the decision that certain stimuli, even when physically intense, will not have drive-like effects on behavior. Referring back to the arguments of the anti-drive-reductionists, it should now be apparent that many of the supposed paradoxical effects of increased stimulation disappear in the light of these suggestions. A secondary reinforcer does involve an increase in stimulation, but not necessarily an increase in drive, and it may, in fact, evoke responses which actually reduce drive. The laboratory world, as seen from a window in a box, represents heightened stimulation for a monkey though not inevitably heightened drive. The masochist stimulates himself in a manner that could be painful and drive arousing for others, but not for him.

The solution we have proposed to the problems raised by the anti-drive-reductionists may appear to have achieved its goal at some sacrifice of objectivity. To assert that the drive-arousing potential of a stimulus is not necessarily correlated with its physical intensity is to maintain, of course, that certain characteristics of the organism must be listed among the elements of a definition of stimulus-produced drive. It means that a particular stimulus will not be drive arousing if an organism has had specific kinds of learning experiences with it, if his sensory receptors are in such and such a state of adaptation, or if he is being simultaneously titillated by certain other strong stimuli. This requirement, that the specification of stimulus-aroused drive include additional information about the state of the organism and its history, does to be sure, add complexity to the drive problem, but it presents no insurmountable obstacles. Drive states resulting from operations of deprivation are subject to the same kinds of restrictive specifications. A 24-hour period of food privation means one thing if the subject is old and obese, and something different if the subject is young and slender; it means one thing if the subject has been on an ad libitum diet for weeks, and something else if he has recently undergone a prolonged deprivation schedule. Drive level, defined either in terms of deprivation time or

stimulation conditions, can be maximally useful only if additional data about an organism's previous experience, momentary states, and responses are taken into consideration.

In the preceding discussion, some of the conditions under which stimuli would *not* be expected to have drive-like effects have been examined in detail. Before concluding, it seems appropriate to consider briefly some instances in which stimuli *would* be expected to exhibit motivating effects. These are cases in which all the requirements of the definition, as amended by statements about previous experience, sensory adaptation, and so on, have been satisfied. But to grant that intense stimuli can qualify as contributors to drive level is to raise the original problem with which this discussion began. An admission has been made that responses followed by intense stimulation may be learned or perpetuated, at least when the stimuli are deleted from the category of drives. But can it be admitted that responses may be learned or maintained when their onset is followed by stimuli that cannot be stricken from the class of drives?

Either an affirmative or negative answer can be given to this question, depending on the nature and strength of responses evoked by the stimulus and on the relation between the time of drive increase and the time of occurrence of the response. The discussion of this matter can be conveniently organized with respect to these temporal relations.

Consider first the case where *the stimulus-produced increment to drive does not appear until after a response has terminated.* Here it is obvious that the drive, though genuine, cannot act to energize or intensify the completed reaction. Indeed, if the interval between the response and the stimulus is relatively long, the increment in drive should have no effect upon either the vigor of the behavior or upon the probability of its recurrence. As is well known, long-delayed punishments are quite ineffective as deterrents to the performance of undesirable responses. Moreover, if the onset of the drive is delayed, its reduction must also be postponed, and the effectiveness of any reinforcing power possessed by that reduction will be reduced. It seems unlikely, therefore, that a response will either be learned or unlearned if it is followed after an appreciable time by both an increase and a decrease in drive. The possibility remains, of course, that learning may occur under the special conditions suggested by Miller and Dollard, where the decrease is substantially greater than the increase, and where the temporal interval between response and stimulation reduction is minimal. It is also possible that when the time interval is short, the drive increment may lead to the abandonment of the response. Such a "punishing" effect might be observed when the cues for the completed response are still present and when the drive-arousing stimulus evokes reactions which are incompatible with the completed one.

Finally, let us examine the situation in which *the stimulus-initiated increment to drive is produced while a response is in progress.* Since the response is actually occurring, the tendency to perform it must be greater than that for any other act. If drive is assumed to intensify whatever response is dominant at the moment, it follows that the ongoing reaction, as long as it is dominant, will be facilitated rather than disrupted.

Evidence to support this contention is found in studies by Muenzinger (17) and Drew (2), who report that electric shock administered after the choice point facilitates learning, and in Ullman's (27) experiment in which the

rate of food consumption increased if rats were shocked while eating. Similarly, Miller (14) found that a momentary shock delivered to rats on the stem of an elevated T maze increased their speed of running and decreased their errors. His rats had been trained to run to food when hungry but were thoroughly satiated at the time shock was administered.

It thus appears that *a step-up in drive occurring during a response can exert a dynamogenic influence on that response, providing the eliciting cues are exercising a high degree of control over the behavior.* Moreover, this magnifying effect should appear on the very first occasion when the drive is heightened, i.e., before the drive-initiating stimulus has had any opportunity to accumulate either associative connections with the ongoing action or any secondary reinforcing attributes. Any stimulus capable of arousing a drive, however, may also be rich in its own potentialities for eliciting specific movements. If so, its presentation may upset the initial dominance relations among the reaction tendencies, and the response in progress may suffer interference. The anti-drive-reductionists seem to feel that a negative effect of this kind should be predicted by their opponents' theory on every occasion when increased drive occurs during a response.

A drive reductionist can admit, therefore, that genuine drive increments can occur after the initiation of a response and that the response may, nevertheless, persist or even become stronger. And he can admit this without having to abandon his special hypothesis as to the mechanism of reinforcement. He merely asserts that the ongoing behavior should be intensified except when it is overridden by competitive reactions. Though he assumes that a reduction in drive is reinforcing, he need not also assume that an increase in drive, in and of itself, produces an obverse negative effect.

In summary, we have been concerned in this discussion with some recent criticisms of the drive-reduction hypothesis of reinforcement. Though these criticisms differ in their manifest content, all are grounded in the observation that when a rise in stimulus intensity takes place during a response, or shortly thereafter, the probability that the response will recur does not always become less, and may even grow greater. If an increase in stimulation is alone sufficient to define heightened drive, then this observation constitutes an apparently crucial exception to the drive-reduction view, because the response is first followed by increased drive and only later by its decline. The fact that organisms often behave so as to obtain more stimulation rather than less, or to obtain so-called "pleasant" stimulation, constitutes, in essence, an identical apparent exception to the drive-reduction view.

In seeking to solve this problem, it has been denied that the drive state, if any, resulting from or induced by stimulation can be usefully defined solely in terms of the sheer physical intensity of the stimulus. The definition must be broadened to include such additional factors as amount and kind of previous experience with a stimulus, level of sensory adaptation, and degree and kind of simultaneous stimulation. When this is done, many of the supposed exceptions to the drive-reduction hypothesis are eliminated, since they can no longer be correctly described as involving increases in drive.

But even when all conditions of the modified definition have been met, some stimuli still qualify as drive arousers. The effects of these upon behavior can be coordinated with the drive-reduction view by contending that if they occur at

appropriate times they will facilitate rather than disrupt behavior. Instances in which disruption does take place can be attributed to the elicitation of interfering responses by the drive-producing stimuli, and not to any negative motivational attributes inherent in them.

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THE FUNCTIONS OF SCHEMATA IN PERCEIVING

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The Development of Schemata

There has been a striking tendency among psychologists in recent years to stress the importance of motivational factors in determining our perceptions of the outside world. Perception has even been regarded as "an approach to personality" (see Blake and Ramsey, 5), as though a study of the individual's percepts would throw some light upon the nature of his individual personality characteristics. These psychologists appear to underestimate the importance of the cognitive aspects of perception; of the parts played by thought and reasoning in building up our understanding of the world; and of the functions of our background of knowledge [1] in determining our immediate percepts. At the same time, these psychologists seem to overestimate the influence of the factors of immediate need, and especially of the relatively transient and unimportant influences they exert upon most of the simpler and more straightforward of our perceptions. It is possible to hold the view that percepts are constructed from sensory data though not wholly a function of them; and at the same time to maintain that the construction is determined by cognitive activities as much as by individual need and interest. I shall proceed to discuss this view.

It is clear to all that at the basis of our perceptions of the external world there are a series of effects in the sense organs produced by external stimulation which vary in an orderly way, both temporally and spatially. And, as Gibson (12) has pointed out, our percepts vary also in gradients which correspond closely to the gradients of stimulation. Moreover, there is an orderly continuity of sensory impression between one moment and the next; and there is also a consistency and correspondence between the types and variations of effect in the different sense organs. From these patterns of sensory impressions are constructed our percepts of the external world. We assume that our percepts represent accurately what is "really there." We believe that certain visual impressions represent solid objects of particular shapes, which continue to exist indefinitely; and this probably because we can repeatedly check the fact of their solidity and identity by touching and manipulating them. For most practical purposes, tactile and kinesthetic data can be relied upon to be consistent.

It seems fairly clear from the evidence of Piaget and others (discussed in Vernon, 44) that the infant is not at first aware either of the orderly repetition and continuity of sensory data, or of the association between such data from different sense organs. Neither does he allocate such impressions to sources in a more or less permanently existing external environment. He gradually acquires the processes of constructing his percepts of the external world through manipulation and experiment. It may well be that the tendency to do so is innate and develops through maturation; but the infant does require specific individual practice and experience before he arrives at the concept of the world around him as permanently existing outside himself

[1] "Knowledge" is used throughout this paper in its widest sense, to include not only verbally formulated knowledge and ideas, but also our background of knowledge of how to behave appropriately in the widely varying range of situations we normally encounter in our everyday life.

and independent of himself. Indeed, the notion of the complete independence and objectivity of the external world seems not to be fully established until many years after birth.

Now is it not obvious that this view of the external world must be constructed by the individual for himself, since it cannot be directly "given" by the sensory impressions coming from the sense organs? And since, sooner or later, our perceptions and ideas about the world do become in general coherent and consistent, is it not probable that they are coordinated in accordance with some system of classification of percepts and ideas about them? If the infant is to be impressed by the repetition, coherence, and consistency of certain combinations of sensory data, he must adopt some organized procedure of retaining, coordinating, and classifying these data in an orderly manner. The earliest classifications of data may be formed in relation to the satisfaction of the needs common to all children; they center upon the mother, who is the supplier of food, warmth, and comfort. But, also, the infant and the young child are intensely interested in the appearance of the phenomena they experience, and make every effort to study and understand them. Thus data are classified in terms of the appearance and behavior of external objects and situations. These classifications supplement and are substituted for the classifications in terms of need satisfaction. As language develops, it forms a tool of increasing value in assisting the former type of classification.

I suggest that to this type of classification we may usefully apply Bartlett's concept of the "schema" (3). Bartlett defines the schema [2] as follows:

> "Schema" refers to an active organization of past reactions or of past experiences, which must always be supposed to be operating in any well-adapted organic response. That is, whenever there is any order or regularity of behavior, a particular response is possible only because it is related to other similar responses which have been serially organized, yet which operate, not simply as individual members coming one after another, but as a unitary mass (3, p. 201).

Thus we may regard schemata as persistent, deep-rooted, and well-organized classifications of ways of perceiving, thinking, and behaving. They are based upon the individual's knowledge of "what sort of thing to do in, and about, certain kinds of situation." Thus they involve the classification of (*a*) situations of related kinds; and (*b*) kinds of behavior appropriate in these situations. But the behavior and its results react back upon the classification of situations. If the results prove unsatisfactory, the classifications of both situations and behavior may be modified until they appear to be more realistic and more appropriate. Thus schemata are not in themselves stereotyped habit systems, though in certain circumstances it may be that they will degenerate into such habit systems.[3] There is no doubt, however, that the individual's motivational tendencies will operate in such a way as to build up and strengthen schemata which lead to behavior that satisfies need and to modify or suppress those which do not.

The essential point to note is that the use of a concept such as that of the schema reflects the fact that our percepts, thoughts, and behavior are on the whole consistent and orderly—reasonably appropriate though not necessarily logical—and that they are consistently related to previous acts of perceiving, thinking, and behaving. Thus,

[2] For a fuller discussion of this definition, and of its relation to other uses of the term "schema," see Oldfield and Zangwill (26).

[3] Recently H. Kay has, in an unpublished thesis, given evidence that old people's behavior shows signs of such deterioration.

in investigating the operation of schemata, we must endeavor to find if the observer shows some consistent method of classifying perceptual situations in a realistic and meaningful manner. Furthermore, we must note if there is evidence that this type of classification has been built up through individual personal experience or interest, in which case there may be considerable individual differences in perceiving the same situation (such as the responses I have quoted elsewhere [**44**, pp. 262–263] to a reproduction of Sickert's picture, "Brighton Pierrots"). Or if the systems of classification are shared by members of a group having, for instance, a common culture or type of education, we may expect that the percepts of these people will be alike but will differ from those of other groups. Lastly, there are many schemata related to the spatial and temporal relationships of objects in the external environment which are probably acquired by almost everyone in the course of his ordinary everyday life. It is not always easy to detect the manner in which perception is determined schematically, since often we are unaware of the background of knowledge and experience to which our present perceptual behavior is related. But it is, I think, quite essential to the understanding of our immediate perceptual reactions to take into account these organized classifications of knowledge about the physical and social environment which guide and inform us as to "what is the sort of thing to do about the sort of thing we now see." [4]

Operation of Schemata in Experiments on Perceptual and Form Qualities

Although the sensory data are accurately recorded at some level in the central nervous system, nevertheless, it does not seem that we are ever directly or consciously aware of anything corresponding to the sensations of Titchenerian theory. Instead, we perceive stable solid objects, enduringly located in the world around us. This "visual world" is perceived, and we react to it, in accordance with the schemata we have built up for ourselves from infancy upwards. But we do also perceive that certain aspects of the external world do at times vary independently of one another, and that they cannot, and must not, always be determined by our ideas of the general nature of the external world and the objects which it contains. Perhaps only the sophisticated Westerner has gone to much trouble to distinguish these particular aspects and their inherent differences and variations. For it is only by a considerable effort of differentiation and abstraction from his total perceptual experience that he can perceive them at all. Moreover, this abstraction is never complete. What is perceived is always affected to some extent by the global schematized perception of the total field; and other aspects of the field are always liable to slip in and distract the observer.

Now it seems that most of the *experimental* work which has been carried out on perception depends on the ability of the observer to abstract more or less completely certain aspects of the perceived field from the total global impression. In particular, all so-called "sensory" experiments are of this nature. They indicate that the trained and sophisticated observer can analyze his perceptual field, and differentiate certain parts of it from the remainder, with considerable finesse. Furthermore, it seems to be true that the degree of possible differentiation is fairly consistent, both in the same observer from time to time, and also between different observers with the same degree of training.

[4] This appears to correspond to Postman's views (**31**) on the categorization of "perceptual response dispositions."

It has been assumed that in such experiments, the abilities of the sense organs to respond to and discriminate stimuli are being measured. It would be more correct to say that when the situation is arranged in such a way that the physical stimulus is varying in one way only, and all other stimulus qualities are maintained constant, then the variations in what is perceived are largely a function of that stimulus variable. But appropriate schemata are nevertheless operating in the construction of the percepts. For they will vary greatly between different observers with different degrees of sophistication and training, and those who have built up appropriate schemata of observation and response will react differently from those who have not. Furthermore, in situations which are not limited in this way, other schemata will operate, and the percepts will be differently constructed from the sensory impressions.

Again, in many experiments on perception, as Gibson (13) has stressed, the observer is required to abstract from his total experience small differences in the outlines of two-dimensionally projected shapes, shown to him momentarily. He learns by experience to construct some percepts of this type with considerable facility. Whether all children learn to recognize representations of objects from their outlines is impossible to say. Certainly children brought up in Western civilizations acquire this facility at an early age, presumably by coordinating together visual impressions of brightness gradients and tactually perceived "edges." But when it is a matter of perceiving nonrepresentational outline drawings, of the kind so frequently used by the gestalt psychologists, the individual has little previously acquired experience to assist him. Then his percepts may be characterized by the failure to differentiate the parts from the whole, as is shown in many of the gestalt figures and in the visual illusions.

OPERATION OF SCHEMATA IN "CONSTANCY" EXPERIMENTS

In experiments on the "constancies," the observer is required to differentiate and abstract aspects of his total experience in a way that is quite unfamiliar to him. The child has learned in the course of his development that objects may retain their identity, and hence their "real" shape and size, although the visual aspects of their shape and size vary as their spatial position varies. This he learns at a comparatively early age with objects which he can reach and handle, but at a much later age, as Piaget (27, 28) has shown, with distant objects which he cannot manipulate. In the end, however, he can make this act of identification even when, for instance, distant objects appear actually as little more than dots on the landscape. Furthermore, on the basis of impressions of projected size and shape, he can and does make fairly accurate estimates of the distance and spatial orientation of the objects. What he is never required to do, and cannot do with any accuracy, under normal conditions, is to estimate their projected size and shape. (Piaget and Lambercier [30] have shown that this is done more accurately by younger children, aged 7–8, whose percepts are less closely linked to the spatial framework, than by older children, aged 9–10; after that, the ability improves somewhat as they become more capable of making abstract judgments.) Yet this is what the observer is required to do in experiments on size and shape constancy. The unsophisticated observer naturally finds this task difficult. In estimating or matching the size of distant objects, he is apt to choose the size which the object would appear to have if it were close to him. He uses both his knowl-

edge of the size of the object as such, and also schematized knowledge relating this size to the retinal projections of objects at distances which can be judged from other criteria. But suppose that, instead of being required to estimate the perceived size of the object, he is asked to estimate what fraction or segment of the total field of view it covers. Then, as Joynson (17) has shown, the estimated size approaches that of the retinal projection of the object. Much the same procedure must be adopted by anyone who attempts to make a landscape drawing. He must try to estimate the size of the spaces between objects, as well as of the objects themselves; otherwise the latter will be drawn too big to fit into his drawing. Thus the object is judged to be of one size if it is regarded "figurally," as an object in itself, and of another size if it is perceived as part of the "ground."

A somewhat different situation seems to arise in experiments on shape constancy. Here the observer rarely judges the shape of the inclined object to be exactly the same as that of the object in the frontal parallel position, unless the angle of inclination is very small. Thus if the object is a circle, his judgments become more and more elliptical as the angle of inclination is increased. It is possible that in the course of our upbringing we learn to perceive two-dimensional shape as such, and apart from the object concerned, with a fair amount of accuracy. The observer attempts, in the experiment, to apply this schematic perceptual category to the estimation of projected three-dimensional shapes. In effect, he says to himself, "This tilted circle looks elliptical." But he cannot judge very accurately how elliptical.

It is not justifiable, I consider, to compare the processes of estimation which occur under normal conditions with those which operate when the object is exposed in such a way that only its projected shape is visible. Such estimates are made when the object is viewed monocularly, with no variations of lighting or surface texture, and with all surroundings concealed. The normal schemata are not brought into action, and the variations of the percept are determined by the only variation perceptible—that of projected shape. In the experiments of Ittelson (15), however, the observer perceived a single shape continually varying in size. His schematized ideas as to the persistent nature of objects, which frequently change their distance but not their size, suggested to him that, when the area of retinal stimulation varied, the object was in fact moving rather than expanding or contracting; and that was what he perceived. Again, in the experiments of Langdon (20), a circular outline shape made of wire was rotated about a vertical axis, in such a way that its projected shape varied continuously. The shape of the object was then estimated as somewhere between that of the circle and the projected shape, thus approaching the estimate that would have been made under normal conditions of perception. Possibly, once more, the continuity of experience was the important factor; and the rotating outline was perceived in somewhat the same way as a stationary solid object whose spatial orientation was clearly visible.[5]

A rather different schematic determination of perception again appears to operate in brightness—or "whiteness"[6]—and in color constancy. Here also it is true that what we are concerned with

[5] Langdon states that it often appeared to the observers that the wire shape was being squeezed in and out, rather than rotated, but that this made no difference to their estimates. It is possible that they developed a procedural "set" in matching which was too well-established to be disrupted.

[6] In accordance with the usage of Thouless (42) and of Koffka (18).

in ordinary everyday life is the intrinsic or normal whitenesses and colors of objects—those which appear in ordinary daylight. Over a wide range of brightness and hue in general illumination, we do ignore changes in perceived whiteness and color of objects. At the same time, if an observer is asked to match the whiteness or color of white objects exposed in dim or colored light, against gray or colored objects exposed in bright white light, it will appear that his perceptions of the former have become appreciably modified—that is to say, they are less white or are tinted with color. Now it may be that although we can accurately differentiate between two whitenesses or two colors placed side by side, we have no schematic categories appropriate to the remembering of absolute degrees of whiteness or color. Anyone who has tried to remember the exact hue of a fabric in order to match it against another, even in white light, will confirm the difficulty of this task. We have, it is true, developed some schematic knowledge of the changes in whiteness and color of objects with changes in brightness and hue of the light falling upon them. It may be, as Wallach argues (47), that we have learned to assess the proportion of incident light which is reflected to us, rather than the absolute amount. Thus whiteness constancy is perceived as long as the proportion or ratio remains approximately constant. But this judgment is not completely accurate, and constancy of whiteness and color is in general less than constancy of shape and size.

I do not of course claim that this discussion of the "constancies" has covered exhaustively all the phenomena they exhibit. This argument merely advances a new approach to the subject, which might suggest a somewhat different type of experimental investigation, for instance, studies of the relation of constancy judgments made in different types of naturalistic surroundings, and with different types of experimental instructions. Those of Gibson (12) constitute a valuable beginning which needs to be followed up. Above all, it is important that the observers should attempt to report in some detail the nature of the phenomena as they actually perceive them, and thus demonstrate the individual differences in their perceptions.

RELATION OF SCHEMATA TO MOTIVATIONAL FACTORS

I have argued that we perceive certain aspects of the perceptual field in accordance with the schematic category of events to which at the moment they seem to appertain. Since these schemata develop differently according to different individual experience, it cannot be assumed that percepts which may be claimed to be, logically and objectively, of the same kind—for instance, the "constancies"—will in fact follow the same logical principles or "laws." Furthermore, the operative schemata can up to a point be varied by the intentions of the observer—by his concern with certain aspects of the field, and his lack of interest in others. The individual can choose to some extent what he will perceive. But it has generally been found that in carrying out skilled perceptual tasks, for instance, the observer cannot perform satisfactorily, however strong his motivation, merely by intending from moment to moment to concentrate on particular aspects of the field. He must be instructed beforehand where and how to direct his awareness, and he must acquire facility in this by repeated practice. In other words, he must build up the appropriate schemata for the selection, assimilation, and interpretation of the appropriate sensory data. The formation of the schemata is strongly motivated and stimu-

lated by interest, but the actual percepts are not directly determined by the motivation. The construction of such schemata is a necessary part of the training of aircraft spotters, radar and Asdic operators; and in a variety of industrial tasks of inspection and of grading materials.

The schemata operating in perception perform two functions: (*a*) They produce a condition of expectation in which the observer is not merely on the *qui vive*, but also knows what to look for—what particular sensory data to select from the incoming flood. (*b*) He then knows how to deal with these data—how to classify, understand, and name them, and draw from them the inferences that give the meaning to the percepts. We are sufficiently familiar with the case of the unsophisticated student who does not even see the minute details of, for instance, biological specimens, from which the expert can derive the most important information. An interesting study of such cases by Johnson (16) demonstrated the manner in which students could be directed to observe the essential points of specimens, etc. seen under the microscope, which they had hitherto overlooked, and not to make incorrect inferences about them. Another instructive example of this phenomenon occurred in an experiment by Fox (11), who showed pictures of armor to students, and required them afterwards to report what they remembered of the pictures. He found that if he gave the students some preliminary instruction on technical details, and if they thoroughly understood and assimilated this information, then subsequent performance was superior to that of the uninstructed. But if, on the other hand, they did not fully assimilate the instruction—that is to say, it had not been fully absorbed and schematized—then it did not assist them at all, but merely confused them.

Now it might be argued that such highly skilled perceptual tasks were characteristic only of the expert, and had little relationship to the perceptions of ordinary everyday life. The distinction seems to be one of degree rather than of kind. It was pointed out above that young children were, in general, intensely interested in and curious about the external world and the objects and events within it. Is it not probable that for many of them the same features of the environment are important and interesting, and that therefore they build up similar schemata on which to base their subsequent perceptions? Thus Piaget has described the development of schematized ideas and corresponding perceptions of movement (29), causal phenomena (27), distance and space (28), and many others. Though his observations covered only small numbers of children in one particular environment, it seems probable that all children must develop such schemata sooner or later in order to adjust themselves to the surrounding world. But insofar as their environments differ, for instance, as between town and country dwellers, then different schemata are also developed.

Such differences in individual experience also seem to operate in what is called "social perception." It is a subject too wide to discuss here in detail. But I would include in it both the ability to perceive and act upon fine shades of behavior and emotional expression in others; and also differences in the construction of percepts from given sensory characteristics. There is little doubt that particular interests and sentiments affect the ability to develop schemata for classifying and responding appropriately to what we perceive in the behavior of others. As regards the selection of particular sensory data, it appeared in the experiments of Marks (24) that individuals with pro-Negro sentiments

had developed schemata which enabled them to perceive and remember in greater detail than those without those sentiments the characteristic facial differences between different Negroes. In the experiment of Seeleman (41), Negroes themselves seemed to have established schematized scales of facial color, which in part determined the estimates of color which they made in the experiment.

It is possible that some long-term schema based upon the interests of a particular social class was operating to cause the overestimation of size of the more valuable coins in the famous experiment of Bruner and Goodman (6). However, the dust of controversy has not settled upon the methods and results of this experiment. It seems fairly certain that, as in the experiments described by Ansbacher (1) and Postman and Page (37), there was an inability to isolate a single aspect from a global percept. But one would need to know more about the poor children to decide why their overestimation was greater than that of the well-to-do.

However, in these and other cases the schemata were very complex, and the factors which brought different schemata into operation from moment to moment are obscure. Although it seems probable that in prolonged and continuous perceptual activity, highly developed and motivated schemata will tend to operate, it can by no means be assumed that any particular schema will function at any particular moment, particularly in somewhat ambiguous and indeterminate situations. In some experiments, such as those on tachistoscopic perception, there may be nothing to inform the observer as to which, if any, of his schemata are appropriate to the particular material presented to him. In such circumstances, schematic determination of the perceptual response may be weak and unclear; no one schema can function very effectively, and the observer may be quite uncertain what to look for and to select from the visual field. He is then prone to develop, as a reaction to the experimental situation, a cognitive "set" which causes him to expect and hence to perceive the type of material indicated by the instructions or the conditions of the experiment. Thus in one experiment (43), I presented tachistoscopically to a number of airmen a series of rather bizarre pictures, terminating in a simple picture of an airman in a cockpit. Several of them failed to recognize the picture, and it seemed that this failure was due to a set established by the preceding pictures to perceive "something queer." In this situation they had no reason to suppose that any particular schema, for instance one related to their professional interests, would be appropriate; thus their perceptions were determined by the short-term cognitive set. Of course, had this type of situation been indefinitely repeated, and had they been satisfied that their responses were appropriate to the situation, the set might in time have developed into a schema related to this situation. Such schemata, as was noted above, may be built up by observers who have had prolonged training and experience in experiments on sensory qualities.

We may now proceed to examine some of the experiments which have purported to demonstrate the direct effects of short-term need states upon perception. It must be noted that in many of these experiments, also, there may have been no schema available to direct, in a clear and unambiguous manner, the observer's expectations of what would appear, or his selection from the sensory data presented. But, indeed, in some of these experiments it hardly seems that we are dealing with perception based upon external stimulation; but rather with the projection of inter-

nal imagery upon the external situation. The latter process appears to have occurred in the experiments of Levine, Chein, and Murphy (21) and of McClelland and Atkinson (22) on the reporting of "food objects" by hungry observers in response to subliminal stimulation, or to no stimulation at all. Yet Postman and Crutchfield (36) have since shown that even a temporary cognitive set established by instructions to expect to see words connected with food was more effective in stimulating the perception of such words than was actual hunger. After all, why should a hungry individual expect to see words connected with food, or even pictures of food, in a tachistoscope? Why should he wish to do so? But there is nothing to be gained by confusing processes which are initiated by sensory stimulation with those for which there is no known sensory basis. This applies also to the Rorschach responses, for instance, which consist of imagery that the observer himself can differentiate from the immediate percepts (cf. data reported in the chapter by Dennis in Blake and Ramsey, 5). Indeed, it is probably true of all situations in which the sensory data are so scanty that the individual is forced to construct something imaginatively. Even if it is impossible to draw a hard and fast line between these situations and those of normal everyday perception, it is still undesirable to extend conclusions based upon the former to cover the latter also.

In many of the genuine perceptual experiments employing the perception of words, it appears that perception was determined by the long-term schemata which direct this extremely subtle and highly skilled activity. In particular, the effect of familiarity, a function of these schemata, is always great. No accurate compensation for individual differences in familiarity can be provided by the use of the Thorndike-Lorge word count, since highly educated individuals vary greatly in their familiarity with the more uncommon words, and it is impossible to measure this variation. Familiarity, however, is probably at least in part a function of schemata built up in accordance with individual interests. The individual develops particular sets of ideas, and words to express them, which are related to his interests. This indeed appeared in the experiments of Postman, Bruner, and McGinnies (35) and of Postman and Schneider (38). Here the tendency was for observers to perceive most readily words, and especially unusual words, which were related to their prevailing interests.

In another type of experiment, for instance, those of Verville (45) and of Verville and Cameron (46), the observer's ability to perceive incomplete pictures appeared to be inhibited as the result of certain frustrating experiences which they had received not long before. The same effect upon the reading of words was demonstrated by Postman and Bruner (34). It seems probable that in these experiments the observer became accustomed to the occurrence of a task which was too difficult for him to accomplish, and in most cases resigned himself to failure. However, Eriksen (10) reported that observers who forgot their previous failures were slower than those who did not, to perceive words which were likely to affect them emotionally—those to which they gave a long reaction time in a free association test. Thus it may be that in some cases an experience of failure may stimulate the observer and set up an increased concentration of attention.

But most of the experiments on the so-called "perceptual defense" have probably been affected by differences in familiarity with the "taboo" words presented. Again the Thorndike-Lorge word count seems singularly inadequate for equating the familiarity of these

words with the familiarity of the "neutral" control words. Familiar acquaintance with "rude" words, the readiness to report them without hesitation, and the whole complex of emotions tied up with them, are likely to vary enormously among different individuals. Indeed, considerable variability in these respects was indicated in the experiments of Bruner and Postman (7) and of Cowen and Beier (8); though Bruner and Postman provided a control on individual differences by measuring the reaction times in free association. That many of the observers did dislike saying the taboo words out loud was clearly shown in the comments quoted by Bitterman and Kniffin (4) of observers whose susceptibilities had obviously been outraged! However, it seems that this effect wore off as the observers became accustomed to the situation, according to the results of Lacy, Lewinger, and Adamson (19). That it could be removed by allowing the observers to write the words, and by giving them special instructions to do so, appeared from the experiment of Postman and Gropper (32). Once these difficulties had been overcome it seemed that, in general, the taboo words were perceived *more* easily than the neutral words, rather than more slowly, as McGinnies (23) originally suggested. Thus it may be that in certain circumstances the ease of perception may be increased by emotional excitement. However, the conclusions drawn from these experiments are of doubtful import, because of the effects of differences in familiarity; and the same may also be true of experiments carried out with pictures, such as those of Eriksen (9). It would be difficult to conclude that these emotional factors determined the actual nature of what was perceived.

We come now to experiments in which it did appear that what the observers perceived was in part a function of motivation set up during the experiment. In the experiments of Proshansky and Murphy (39) and of Schafer and Murphy (40) the observers were found to accentuate certain aspects of what they perceived and to reject others, insofar as the former were presented in conjunction with a "reward," and the latter in conjunction with a "punishment." For the reward money was given them; and for the punishment the money was taken away from them. The experimenters satisfied themselves that the observers did not become consciously aware of the connection between the particular aspects of the field, and the reward or punishment, so that they did not deliberately choose the former and reject the latter. But it is important to note that in the experiment of Schafer and Murphy, the rewarded aspect was favored by contrast with the punished only over a limited period. After about sixteen responses, the observers developed cognitive sets based upon the nature of the experimental material, which thereafter completely determined what they perceived and eliminated the effects of the reward and punishment. Thus, in these experiments the reward and punishment situation did for a time affect the observers' selection of certain aspects of the field. But the effects were apparently unstable; and there was a tendency for the observer to develop a cognitive set which was more stable and effective. This result parallels that of Postman and Crutchfield (36) with the food words. Haggard and Rose (14) tried to show that the amount of additional activity carried out by the observer in connection with his perceptual activity was also a factor of importance; and it may be that such activity would be related to the establishment of sets. But the evidence was obtained from experiments on the "autokinetic effect," a phenomenon which can-

not be termed wholly perceptual since it is at least partly illusory.

Stronger evidence of the direct effect of motivation upon perception is given by the experiment of Postman and Brown (33). They found that observers who had been told that they had "succeeded" (surpassed their level of aspiration) in certain tasks were relatively quicker than those told they had "failed" (fallen short of their level of aspiration) to perceive words related to success (excellent, succeed, perfection, winner); whereas those told they had failed were quicker than those told they had succeeded to perceive words related to failure (failure, unable, defeat, obstacle). Here it seems unlikely that the results could be attributed to differences in familiarity with the different words. Moreover, the experiences of success and failure were set up in the experimental situation and did not reflect any inherent individual tendencies to optimism or defeatism. There is at present no evidence as to how this effect upon ease of reading these particular words would compare in extent or permanence with that of a cognitive set or established schema. But the motivation had at least a temporary effect upon the observer's selection from the sensory data supplied to him.

The fundamental criticism of the experiments designed to show the direct effects of motivation upon perception is that they do not appear to take into account what this motivation might make the observer expect to see. As we have noted, in situations closely linked to an established interest, and those in which the observer received some specific training, he is on the lookout for certain aspects of the field of view and thus perceives them readily. But in the experimental situations, he may not know what to expect; and he has no reason, in most cases, to expect that anything related to the satisfaction of his needs will appear. Thus sets are readily established by the experimental conditions to perceive certain phenomena which are stressed by these conditions. To determine more clearly the effect of motivation, further thought and experiment are required to work out what are the aspects of a complex perceptual field which an observer in a state of unsatisfied need might look for and select. It may then appear that those features are the ones which have become schematically related to the need state and to the behavioral responses to the field which tend to satisfy the need.

CONCLUSIONS

It is true that we cannot at the moment predict the exact mode of operation of the schemata of any given individual in any particular set of circumstances. Neither can we hypothesize as to their neurological basis; although Oldfield (25) has recently advanced a suggestion as to how they might be explained in terms of the circuital storage devices of modern computing machines. But even in default of such an explanation, I would argue that the concept of schemata and their functions is of value in systematizing the following observations:

1. Percepts of the surrounding world, the objects it contains and their interrelationships, are not infinitely variable, sporadic, and inconsistent. Neither do the percepts vary in a one-to-one relationship with the sensory impression produced by external stimulation, although there is obviously a close correspondence.

2. Percepts are consistent; they have a continuity of meaning and make sense. The meaning and the sense are established by the coordinated knowledge of the observer about the external world and its happenings, and about the types

of behavior appropriate and satisfactory in different perceived situations.

3. The percepts of different observers of the same situation may in certain circumstances differ greatly from one another. But these variations do not appear to be purely random ones; there is some correspondence between the modes of perceiving of any given observer. Different observers have built up different schemata in accordance with their differing individual experience and the interests which lead them to seek this experience. Their expectations and their selection of what they perceive are in part determined schematically. But far more experimental evidence is required to work out these relationships in detail; evidence which must be obtained by a careful study of individual cases, their particular knowledge and interests.

4. In many of the experiments on perception, the observer is presented with a situation in which his existent schemata cannot operate freely, nor tell him what to look for. The sensory data are transient or ambiguous, and cannot be readily fitted into existing categories of experience. In these circumstances, the observer is rather readily influenced by short-term cognitive sets based upon the actual conditions of the experiment. In suitable conditions, such sets may ultimately develop into long-term schemata. His perceptions may also be affected by temporary need states, though I have argued that these effects have not been as clearly established as some experimenters have maintained. Particularly, we have little evidence as to what an observer in such states of need might look for or wish to perceive in any particular experimental situation. It is therefore difficult to draw any conclusions from these experiments which throw light on the fundamental relations between perception and motivation. In contrast, in the perceptions of everyday life, schemata established by previous experiences are available to direct us in perceiving certain aspects of the environment and reacting to them appropriately. It would, therefore, be most valuable to study the consistent modes of perceptual behavior which are shown by different individuals in situations more like those of everyday life than the situations encountered in so many perceptual experiments.

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SYMPOSIUM ON THE PROBABILITY APPROACH IN PSYCHOLOGY *

REPRESENTATIVE DESIGN AND PROBABILISTIC THEORY IN A FUNCTIONAL PSYCHOLOGY [1]

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The movement in psychology most directly concerned with the challenge of unification with the exact sciences is behaviorism. At the present time, two major factions are discerned within behaviorism. One, represented chiefly by Hull (26), tries to elaborate classical behaviorism into a tightly woven, formalized system of strict laws about intraorganismic processes in the nomothetic tradition of physics. The other, inaugurated chiefly by Tolman and his program of molar or "purposive" behaviorism (47), attempts to deal with behavior "relative to some end" and thus to restructure behaviorism in the tradition of the utilitarianism of Darwin and of early American functionalism.

It may be argued that nomothetic behaviorism overexpands physicalism beyond the necessary observational and procedural core and includes unessential borrowings from the specific thema of physics. A functionally oriented objective psychology, on the other hand, dealing as it does with organism-environment relationships at the more complex level of adjustment, may be seen as falling in line with a more searching interpretation of the historical mission of psychology. The present paper concentrates on summarizing and expanding earlier contentions (2, 7, 8, 48) to the effect that the environment to which the organism must adjust presents itself as semierratic and that therefore all functional psychology is inherently probabilistic, demands a "representative" research design of its own, and leads to a special type of high-complexity, descriptive theory.[2] This program provides not only the necessary thematic diversification from the classical

* The series of papers by Egon Brunswik and by Leo Postman, and of the following discussion papers by Ernest R. Hilgard, David Krech, Herbert Feigl, and Egon Brunswik, are adapted from the first part of a symposium held, under the same title and under the chairmanship of Edward C. Tolman, at the *Berkeley Conference for the Unity of Science,* University of California, July 1953.

A contribution by Kenneth R. Hammond to a second, more practically oriented part of the original symposium will appear in the next issue of this journal (20).

The Institute for the Unity of Science in Boston, which sponsored the conference, has contributed approximately one-half the publication cost of these papers.

[1] The present paper has been considerably expanded beyond the original exposition read at the symposium of which it was part. However, care was taken not to alter the substance of the argument on which the subsequent paper by Postman and the ensuing discussion are based.

[2] The expansions beyond the earlier publications listed concern mainly the use of a behavioral example at the beginning of the paper; the brief consideration of such semirepresentative policies as "canvassing"; certain comparisons with factorial design and the analysis of variance, as well as with nonfunctionalistic uses of probability in psychology; and a discussion of actual and potential applications to the clinical-social area and to related domains.

natural sciences but also leads to the long overdue internal unification of psychology.

We will develop our arguments first with the use of an example involving behavior as a "constant function" of a characteristic end state, and then in reference to the functionalism of the perceptual constancies where progress along methodological lines is somewhat further advanced.

SYSTEMATIC DESIGNS AND THE STUDY OF DISTAL ACHIEVEMENT

Behavior as constant function. One of the earlier functional behaviorists, Holt, suggested that the movements of an individual be defined in terms of "that object, situation, process . . . of which his behavior is a constant function. . . . So in behavior, the flock of birds is not with any accuracy, flying over the green field; it is, more essentially, flying southward" (24, pp. 161–166). This statement, rather paradigmatic of functionalist modes of thought, involves selective description. The preferred hypothesis contains reference to a remote end (south); or, in the words of Heider (21), it sets "distal determination" over "proximal determination," that is, in our case, over description in terms of momentary position (green field).

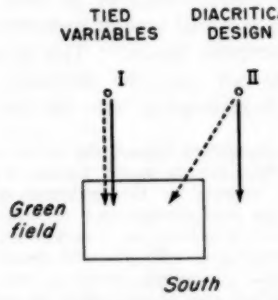


FIG. 1. Systematic designs in the study of behavior constancy (applied to an example from Holt, 24).

In defending his seemingly teleological attitude Holt points to the emphasis some positivist physicists—we may think primarily of Mach—place on the relatively descriptive study of "functional relationships"; these may connect events over space or time regardless of the traditional tracing of causal chains in near-action.

Tied variables. Functionally irrelevant generalizations. Holt's problem of constant function involves generalization. One of the most time-honored traditions in experimentally testing generalizability demands that one, or perhaps a few, conditions be varied in a planful manner decided upon by the experimenter while all others are held constant. The purpose is to assure isolation of the so-called independent variables. For their arbitrary orderliness and confinement such designs may be called "systematic."

For example, we may move the birds backward along the line of their flight, say, to position I in Fig. 1. The birds may persist in their original direction and in this sense show generality of behavior. But it is easily seen that this experiment is irrelevant to Holt's chosen alternative. The two directions, "southward" (solid arrow) and "over the green field" (broken arrow) coincide; allowing for all possible types of response, the two variables involved—south vs. non-south, and green-field vs. non-green-field—are perfectly correlated and thus inseparable so far as the available evidence is concerned. This constitutes artificially induced perfect confounding, and may be labeled "tied-variables" design or, in short, tied design.

Responder replication, that is, repetition with new individuals or with the original individuals at other occasions, is likewise irrelevant to Holt's alternative, regardless of the interindividual or intraindividual consistencies that may

be observed. This must be pointed out in view of a rather deeply ingrained trend in psychology of throwing the entire problem of generalization and of statistical significance onto the responder rather than onto the situation (7, pp. 36 ff.; further drastic evidence to this effect, unearthed by Hammond [18, 19], will be referred to in the section on clinical application).

Diacritical confrontation. Splitting of tied clusters vs. isolation of variables. The real testing of Holt's preferred south hypothesis does not even begin until adherence of behavior to the southward direction is made situationally incompatible with adherence to the green field. This could be achieved, say, by moving the birds sideways to position II in Fig. 1. The two arrows issuing from the new starting point are now divergent rather than parallel. The responder is placed at the crossroads and forced to take sides. The previously tied situational factors are now "confronted" (2); we may speak of this variant of systematic design as "diacritical" design.

We soon discover that southwardness is still tied to such factors as the general area of start, temperature and other climatic conditions, topographical landmarks, magnetic cues, and so forth; and so is the greenness of the field to its squareness or size. What we have accomplished in diacritical design is to separate or "split" one original encompassing cluster into two subclusters of tied variables; but we have not really "isolated" our variable as it may have seemed at first glance, and therefore are not yet entitled to speak of its attainment as a constant function.

Perception as constant function. A concrete perceptual problem comparable to Holt's problem of the constancy of southward flight is that of size constancy. The alternative here is between invariance of the response relative to the measured (or computed) sizes of the stimulus impact at the retina (or at a parallel photographic screen) as the proximal variable, which we will call *P*, and invariance relative to the measured sizes of physical bodies underlying this impact as the distal variable, *B*. Size constancy involves the predominant focusing of the response on *B*.

Classical psychophysics as pseudo-univariate design. With distances variant as they are in daily life, there is a certain degree of statistical independence between *B* and *P* despite the existing causal nexus between them. During the classical phase of psychophysics, however—still strongly in evidence today—the implicit design policy was artificially to tie the distal and proximal variables. For size this is achieved by holding the distance from the observer constant. A good example is the Galton bar. The task is molded closely after ordinary physical length measurement except that the lines are laid up lengthwise rather than being superimposed. This creates a tied-variables design which is comparable to case I in Fig. 1. Note that the tying of the two variables is the direct result of a celebrated device of systematic design, the holding constant of a third variable (in our case, distance).

This design may also be cast into the form of a table of presence, or scattergram. Figure 2 presents the major systematic designs in their minimal form, assuming only two levels of strength for each of the situational variables. In the case of the classical tied-variable design the two variables are perfectly correlated. All entries lie along a diagonal. Photographic size (assumed to be plotted vertically as in Fig. 3) is always large when bodily size (plotted horizontally) is large, and vice versa. And judgments correct (or incorrect) concerning one of these variables are

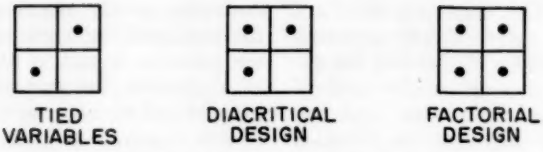


FIG. 2. Minimal scattergrams of systematic designs. Repeatable in k dimensions.

automatically correct (or incorrect) concerning the other, just as Holt's birds, by flying south from position I, automatically must fly over the green field. The classical design therefore precludes decision as to whether the response focuses on proximal (photographic) or distal (bodily) size; nor does it allow the conclusion that the response focuses on either, for that matter.

In the classical phase, the tying of variables was done inadvertently and in considerable naiveté as to the interpretational consequences involved. Since only the bar was directly manipulated and only one scale was read in doing so, the impression prevailed that there was only one independent variable. The notion of univariate design was somewhat obliquely reinforced by the tendency to confine the concept of stimulus to the proximal stimulus variable that had been arranged to vary in unison with the bar. The possibility of varying degrees of dependent variability within the design itself was thus ignored and no provision was made to state the relationships explicitly, so much so that the term "dependent variable" could become synonymous with the organism's eventual responses. Since classical design purports to be univariate yet fails to isolate the distal from the proximal variable, it may be called "pseudo-univariate" design, and in our particular example may be specified as proximo-distally neutral design.

Thing constancy research as a form of multidimensional psychophysics. Factorial design. The diacritical confrontation of distal and proximal size is achieved by removing one side of the bar to a different distance from the observer, say, a smaller one. This has an effect on design analogous to the moving of the birds from position I to position II in Fig. 1. It is now incompatible for the two lengths to be equal bodily and photographically at the same time. The new combination of a relatively small bodily size with a relatively large photographic size injected into the design is plotted in the upper left corner of the center chart in Fig. 2. The forking or parting of the ways which is characteristic of diacritical confrontation may be visualized by assuming the near object to be the Standard and the far one the Comparison, the latter being left to vary along the diagonal.

By projecting the Standard two ways, along a horizontal and along a vertical line, to their respective intersections with the diagonal representing the Comparison series, we obtain two points of objective equality (or POE, formed in analogy to Woodworth's PSE for point of subjective equality [see **7**, **53**]). One represents the variable P and the other B in the experiment. By contrast, in a classical experiment the Standard coincides with a point on the diagonal and thus the two POE's merge in one point, further supporting the erroneous impression of univariate design of which we have spoken above.

The relative allegiance of the response to B or P, or the degree of perceptual "compromise," may be ascertained by inserting added values between (or beyond) the two

ideal "poles of intention" (2) along the diagonal (7, p. 17, Fig. 4). The constancy ratio (called Brunswik-ratio by Woodworth) is a simple device to project the obtained PSE's onto the span between the two pole POE's.

Note that the Comparison in psychophysical experiments has often been called *the* "Variable," so that not only classical psychophysics but even our present diacritical experiment may give the superficial impression of unidimensionality, in spite of the fact that the presence of two POE's clearly marks it as a case of multidimensional psychophysics. In this sense we may say that while classical design is pseudo-unidimensional, diacritical design is crypto-multidimensional.

By adding a fourth point, diacritical design becomes the well-known factorial design (14), also shown in Fig. 2. In essence, this is no more than adding a mirror image to diacritical design, with added advantages accruing by virtue of the increased symmetry. Some of the Vienna constancy experiments have employed such a double diacritical design (2, pp. 167 ff.) but they will not be discussed here.

Variate packages and the indefinite regress of systematic design. Diacritical confrontation or factorial design may be carried to several dimensions. Their number is, or at least should be, known to the experimenter; it may be designated by k.

In analogy to what we have said about Holt's example of bird migration, all results remain contingent upon the ties existing within the more or less incidental situational instance from which systematic variation has taken its start. If we use the term "variate" for the specific values along the various variable dimensions, each concrete situation may be regarded as a "variate package." Originally, each of the particulars or variates in the package has equal claim for being singled out in the description, and one or a few factorial separations remove but little of this indeterminacy. Complete "systematic" isolation of one variable as the crucial factor would involve diacritical confrontation with a very large, and in fact indefinite, number of originally tied situational variables.

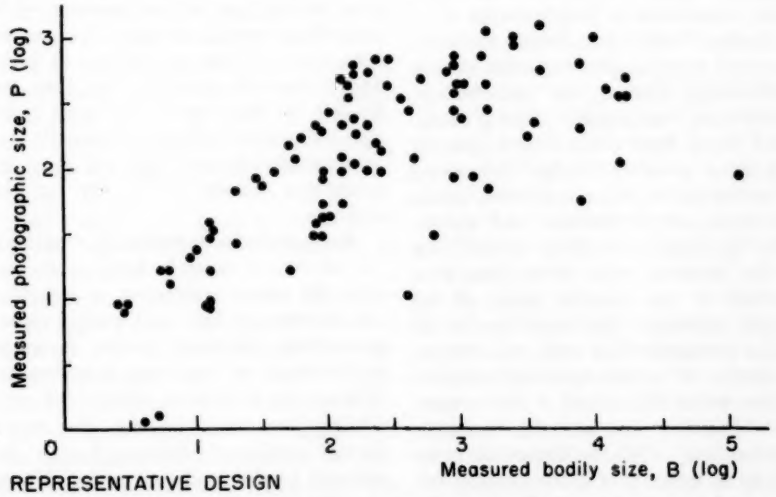


FIG. 3. Scattergram of an example of representative design used in the study of size constancy (adapted from Brunswik, 6, 7). Analogously in x dimensions.

REPRESENTATIVE DESIGN IN THE STUDY OF DISTAL ACHIEVEMENT

From a purely formal point of view the systematic confrontations would have to include the indirect cues for size and distance on which the mechanism of constancy must depend. But it is evident that setting the distal variable against its own instrumentalities would cut out the ground from under the very function whose constancy is to be put to test. Tolman (47) has stressed that all behavior requires the presence of means or "behavior supports." He has also pointed out that the only admissible operational criterion for the testing of "purpose" is the observation of the actual reaching of the end, at least in part of the behavior instances in the class under consideration. But reaching the south or any other distal goal, be it behavioral or perceptual, can obviously become a more or less stabilized function only if the flight of the birds is allowed to take adequate advantage of the natural resources of orientation and locomotion, much as such man-made stabilizers as gun sights must be tested under conditions of practical use.

Ecology and situational instance. Constant psychological function thus is intrinsically limited, or probabilistic, rather than "universal." Flying southward, being right about object sizes, or any other gross or "molar" behavioral or perceptual function can never attain the status of an ironclad and universally applicable so-called "strict" law in the sense in which these laws were idolized in the classical phase of the natural sciences. The basic aim of our initial quotation from Holt requires delimitation of a more specified universe within which the animal is set to operate. This is in line with the "syntactic requiredness" (22) to define all probability in terms of a corresponding reference class or universe. In line with biological usage we will call this universe "ecology." An ecology is defined as the natural-cultural habitat of an individual or group, but is otherwise free of contamination by the system of specific responses. Rather, the ecology is the objective, external potential offered to the organism for survival and its subordinate needs. Nourishment value of foods, as it exists prior to and regardless of its recognition or consumption by the responder, is an example of an ecological variable or set of variables; object size and its system of cues enters via its relevance for manipulation or orientation.

Since the responder merely acts like a catalyst in the definition of the ecology, the ecological environment is not to be confused with Lewin's psychological environment or "life space" which is defined as the reflection of a situation within the response system (see 5).

Situational instances in an ecology are analogous to individuals in a population of responders. Both may be considered as sets of more or less incidental variate packages. The difference is that instances can be taken apart and created at the spur of the moment while individuals usually cannot. But, as we have come to see, a program of functional research demands that they too be left as they come. We must resist the temptation of the systematic experimentalist to interfere, and must introduce a laissez-faire policy for the ecology.

Representative sampling of situations. As we cannot possibly hope to encompass the entire population of individuals in research, but must sample representatively, we must sample instances in the study of functional achievement. Taking the cue from differential psychology, we may transfer the entire formal statistical instrumentarium developed in the study of personality to functional problems as a new content. This will assure, to any desired degree

of approximation, a balanced view of psychological function as it comes about by a synopsis of performance under comfortable conditions, manageable vicissitudes, and a due proportion of risks or well-nigh insurmountable odds.

In terms of experimental design there results a combination of constraint and license in which the experimenter is in no more than supervisory control over the adequacy of sampling. There will be a limited range and a characteristic distribution of conditions and condition combinations. If in this manner psychological experiments take on the character of statistical surveys, we may speak of "representative design" (7).

A representative design in perceptual size constancy. Since in representative design the accent is on sampling from an ecology and on the generalizability of functional constancies to this ecology, rather than on sampling and generalizability in reference to a responder population, it was deemed advantageous for demonstration purposes to confine a pilot survey of perceptual size constancy to a single subject. Using n for the number of responders and N for the size of the situation sample we thus had $n = 1$ subject, a graduate student in psychology; there were $N = 93$ object situations (6; 7, pp. 41 ff.). The objects were sampled, in a reasonably random manner, from the sizes that became "figure" to the subject in her daily routine, in package with their natural setting and accompaniment of depth cues.

Ecological validity and dependent variability within the design. Textural ecology. As in all representative design, the design in itself has the character of a result, even though this result concerns the ecology only and is no more than the precondition of psychological investigation proper. The design obtained for our size constancy survey is shown in Fig. 3. The two major distal and proximal stimulus variables are plotted logarithmically, B in terms of millimeters, and P in terms of millimeters at an assumed projection distance of 1 meter.

The manner of covariation, defining what we have called dependent variability within the design, may in first approximation be expressed by a correlation coefficient. In our case the Pearson r between the logarithms of B and P is .70. This coefficient estimates the cue potential of the proximal variable P relative to the distal variable B in the given ecology, in analogy to the way the validity of a test relative to a personality trait may be ascertained for a population of responders. A correlation between ecological variables, one of which is capable of standing in this manner as a probability cue for the other, may thus be labeled "ecological validity." The study of ecological validities, being bivariate correlational, defines what we may call a structural or textural ecology, in contradistinction to the emphasis on unidimensional distribution (of temperature, precipitation, size of population centers, etc.) which is more typical of biological and a part of cultural ecology.

Our particular coefficient indicates that large retinal impacts are somewhat more likely to be caused by relatively large objects, regardless of distance. Considerable as this relationship may seem, it is, as we shall see in a moment, trifling in comparison with the final achievement of the constancy mechanism. This gain becomes possible only by an additional utilization of distance cues. For some of the less valid of the commonly listed perceptual depth criteria, such as vertical position, subdivision of space, and brightness, Seidner has found moderate ecological validities ranging to about .4 (report on preliminary data in 7, pp. 47 ff.). It is easily seen that not even the so-called pri-

mary depth cues, such as binocular disparity, are foolproof in our ecology. For example, binocular disparity is present in the stereoscope, yet depth is absent in the underlying reality; in viewing reality through a camera, on the other hand, binocular disparity is absent while depth is present in the chain of causal ancestry.

In our present example the analysis was not carried to an explicit treatment of depth criteria and other context factors. With the use of photographs, such as those Seidner had available for his analysis of depth cues, a great variety of them could be analyzed in a single enterprise. In fact, Fig. 3 must be seen as combining but two out of a practically unknown number, x, variables. Since covariation must be allowed to take its natural course, the different juxtapositions would of course not look exactly alike, as they do in factorial design, but would merely be analogous, forming in the end an x-dimensional space in which all factors could be considered simultaneously.

Quite aside from the avoidance of the pitfalls of systematic design—in which all factors held constant are lost for the investigation, and the resultant tied-variable clusters only lead to confusion—representative design, while cumbersome and laborious, is thus potentially a very economical technique.

Projections of the frequencies in Fig. 3 upon the main axes are shown in Fig. 4. Our sample, restricted to sizes not tilted into the third dimension and thus by-passing the problem of shape constancy, is represented by the solid curves. It is reassuring to find the distribution for B fairly normal. (The third graph shows the distribution for distance as related to a dependent distance axis that may be imagined to run across the upper right-hand part of Fig. 3 under 45°, forming a triangle with the main axes; the crowding of both small and intermediate objects along the

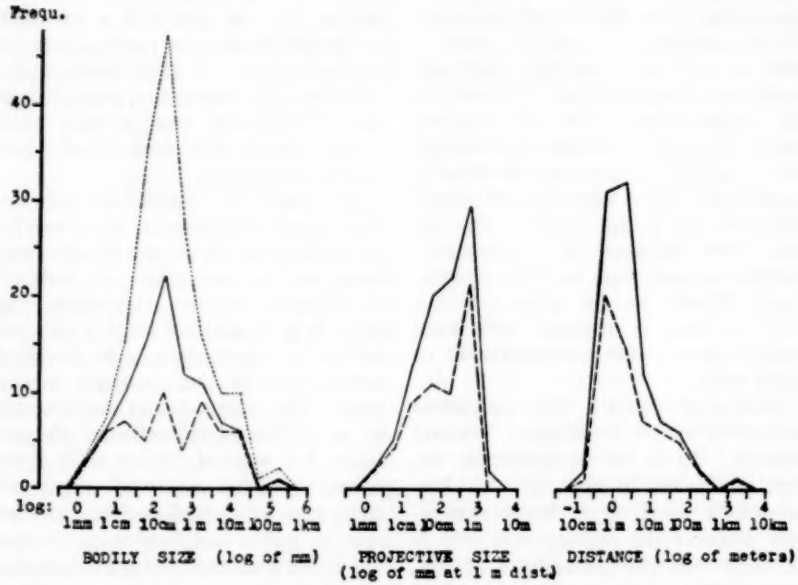


FIG. 4. Frequency distributions of three ecological variables for the representative design shown in Fig. 3 (solid curves; the dotted curve includes tilted objects and the broken curves refer to a subsample of vertical objects, both not discussed here). (From Brunswik, 6.)

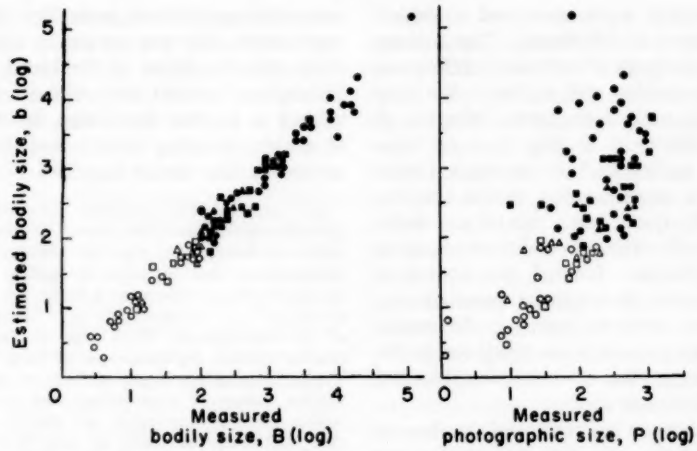


FIG. 5. Scattergrams of perceptual achievement (functional validity) for a subject responding to the representative design shown in Fig. 3. (Open symbols are used here to designate objects smaller than 10 cm., see below; the differences in shape are irrelevant in the present context.) (Adapted from Brunswik, 6, 7.)

10-inch limit of near vision leads to skewed distribution and is the chief source of the curvilinearity that may be observed in Fig. 3.)

Functional validity (achievement). We now turn to covariation between the distal stimulus and the response variable which will define perception in its approximation to "constant function" as understood by Holt. Figure 5, left, shows the stimulus-response scattergram for the logarithms of the perceptual estimates, *b* (so labeled because they were given in the natural, naive-realistic attitude toward bodily size, *B*) as plotted against the logarithms of *B*. The entries cling fairly close to the diagonal, much more closely than the major cue variable, *P*, was found to cling to *B* in Fig. 3. Appraising the maximum errors committed in the particular estimates, however, we find that some entries deviate as much as one-half of a power of 10 from the diagonal, corresponding to about three- to four-fold over- or underestimation of the length in question.

A generic summary description of the degree of perfection of size constancy may again be sought in the correlation coefficient. This represents what may be called the "functional validity" (7) or "achievement" (*Leistung*, 2; for definition see 5, p. 255) of the class of responses, *b*, relative to the distal stimulus variable, *B*. For the total sample of 93 situations this correlation is close to .99 (more accurately, .987 when computed from ungrouped logarithms with three significant places [see 7, p. 44]), in spite of the occasional sizable errors referred to above.

By contrast, the functional validity of *b* relative to photographic size, *P* (Fig. 5, right), is only .73; in addition, this is quite close to the purely ecological association of *B* and *P*, .70, so that little independent focusing of *b* on *P* is indicated. Perceptual restructuring toward photographic size (painter's attitude, p) raises the correlation of the response with *P* no more than to .85. Other attitudes, deliberately inviting intellectually controlled judgment ("betting") and thus the "stimulus error," will not be discussed here, nor shall we go into the question as to whether or not the estimates *b* and p are purely perceptual.

Ecological replication and ecological significance of differences. The analogy to the statistics of individual differences may be carried still further. We may consider each new chance selection of items like those in Fig. 3 as an "ecological replication" of the original sample. In representative design the traditional quest for repetition under "identical" external conditions ceases to be legitimate. Instead, we must seek comparable conditions as drawn from a common universe, just as in differential psychology there is no repetition in the strict sense when other or even the same individuals are used.

Problems of the ecological significance of differences in the statistical sense may be raised, and handled, accordingly. The size of the sample to be used in computation is now given by the number of situations, N, rather than by the number of subjects, n. In this sense the ecological generalizability of distal rather than proximal focusing, in the area of size constancy with distance variant, has been established for our one subject; and even the more crucial in a wide array of relatively moderate ecological validity coefficients of depth cues, referred to above from the material of Seidner, have been found significantly above zero.

In the strict, technical sense, our representative survey of size constancy is completely void of interindividual generalizability. An approach to responder generality was made by using the recorder as a second subject; his distal functional validity was .993, quite close to the .987 for our subject. Furthermore, Dukes (12) has independently obtained a correlation of .991 for a six-year-old boy, using a different sample ($N = 67$) and a somewhat different technique.

It may well be that in many contexts individuals in a population are more homogeneous or stereotyped than are situations in an ecology, and that the ascertainment of ecological generality may be a more challenging task than that of responder-populational generality. Harvey needed only one person to demonstrate the circulation of the blood, and Ebbinghaus needed only himself as a subject to lay the foundation for much of modern learning theory, ecologically narrow as this theory may be.

To complete our analogies between responder-populational and ecological generalization problems, we may set physical size measurement and intuitive or critical estimates of size in analogy to a battery of tests given, not to n persons, but to N situations of the environment. Each type of observation or attitude represents one of these tests. The problem of the degree of "objectivity" of various classes of observation, and thus of certain scientific approaches, can then be handled statistically in terms of their inter- or intraindividual "observational reliability" as tests, in which a sample of situations has taken the place of a sample of persons (see 7, p. 33 and 8, pp. 11 f.).

Representative separation and mathematical isolation of variables. It will be remembered that under systematic design a true isolation of variables can be achieved only by a virtually infinite series of diacritical confrontations or factorial designs piled upon one another. Representative design, while not laying claim to full isolation, separates variables to the extent to which they are separated in the particular ecology but no further, and does not tolerate any artificial perfect tyings (or untyings) between variables. Variables may thus be said to be "representatively separated." From a systematic point of view, a good deal of spuriousness remains built into the textures studied under this policy, samples as these textures are of an ecology that likewise is complexly textured. Representative design is not afraid of this spuriousness; in fact, it welcomes it for the sake of the behavior supports it allows in the execution of the functional approach.

The challenge of further isolation must be met by after-the-fact, mathematical means, as in the study of in-

dividual differences. For example, we may use partial correlation as a mathematical means of holding constant a certain variable. Partialing out P from our above correlation between b and B (and thus in effect reducing the ecological validity of P from .70 to 0) still yields a functional validity as high as .98; whereas factually eliminating the sizes under 10 cm. in Fig. 3—to the right of log $B = 2$—and thus reducing the ecological validity of P from .70 to .14 in a quasi-systematic move, reduces the functional validity to .95 (see also **7**, Fig. 9 and the accompanying text).

It must also be noted that, in contradistinction to systematic design, the process of analysis may be stopped at any point, falling back on the nonreductive aim of functional research, together with the assurance that the unresolved part of the associations is safely within the fold of the ecology to which the investigation has been geared from the beginning.

SYSTEMATIC EXPERIMENTS WITH REPRESENTATIVE FEATURES

We now turn to certain experimental policies, some of them common, which may be considered transitory between systematic and representative design. Representative features may be injected in otherwise systematic designs in a variety of ways.

Quasi-representative choice of variables and of their variation or covariation. Some measure of representativeness may be achieved by the choice of variables with particular life relevance, such as "value," as a factor in constant function. Since ecology embraces cultural norms held valid by the law enforcement policies of a society, along with those connected with physical law or geographical contingency, monetary value becomes a challenge to perceptual attainment on a par with other object properties. Cognitive considerations of this kind, rather than the emotive or motivational aspects of value, per se, prompted the study of what could be called perceptual value constancy. In experiments concerning the apparent numerosity of stamps and of coins, Zuk-Kardos and Fazil (reported in **2**, pp. 140–150) as well as Ansbacher (referred to in **7**) found number constancy with value variant and value constancy with number variant fairly high, although tainted with compromise between the two variables.

Another quasi-representative step is to gear the manner of variation or of covariation between variables to the general scheme of natural conditions in a planfully controlled way, as when the association between a certain cue or means and the object or reward is made probabilistic rather than absolute (**4**, **10**).

Experiments centered about an exemplary instance. Successive omission vs. successive accumulation of cues. A certain effort toward representativeness is discernible whenever a "lifelike" situation is taken as the starting point of the experiment. In the field of size perception such experiments are likely to abandon, at least in some of their phases, the chin rests, darkrooms, screens with small openings, alleys of edges without thickness, or other laboratory paraphernalia in vogue during the late nineteenth century. This liberalization owes much to David Katz (**29**) and his work in another area of perceptual constancy—color constancy with illumination variant. For size constancy, a study by Holaday (**23**; see also **7**, p. 23), and the studies by Holway and Boring (**25**), by Gibson (**16**), and to some extent one by Joynson (**28**) have proceeded by essentially the same functional scheme.

In each case there is what we may call a core or "exemplar" situation—somewhat arbitrarily chosen, to be sure

—which contains a fairly natural array of cues. Superimposed upon this core is a greater or lesser number of systematic variations so that the design is still fundamentally systematic in its ramifications. The systematic part usually effects what may be called "successive omission" from the originally unknown, or at least not fully scrutinized, array of cues, while the more nearly classical approach tends to eliminate all natural cues, then building up from nothing in a technique of "successive accumulation" (7, pp. 22 ff.).

By and large the functional studies of size constancy have borne out Hering's old assumption of "approximate size-constancy" according to which more distant objects tend to be slightly underestimated (although not nearly as much as would correspond to the shrinkage of their photographic size). But it is significant, from the methodological point of view, that in some of the studies the most favorable cue conditions yielded on the average slight over- rather than undercompensation for distance ("overconstancy"), and that the over-all level of "compromise" varies considerably from study to study. In addition, laboratory experiments inadvertently employing certain atypical contexts or backgrounds will yield drastically different results, not to speak of essentially traditional experiments that programmatically employ grossly distortive configurations, like the situations recently created by Ames (27).

Under the aspect of representative design, all these systematic experiments must be viewed as ecological single cases or "instances" with artificial elaboration that leaves a large portion of the core elements untouched. Each of these experiments is indisputable in its results, but at the same time is of unscrutinized ecological generalizability. The variate packages constituting these experiments or experimental settings may be projected somewhere in the x-dimensional manifold of representative design which we have exemplified for two dimensions in Fig. 3. Each imaginary point, or small, orderly group of points, in such a space represents a potential systematic experiment. Mostly there is little technical basis for telling whether a given experiment is an ecological normal, located in the midst of a crowd of natural instances, or whether it is more like a bearded lady at the fringes of reality, or perhaps like a mere homunculus of the laboratory out in the blank.

As a matter of principle, individual sample situations, no matter how lifelike, cannot answer the funtional problem as to the degree of perceptual constancy, even though by the use of responder replication or by systematic variation their results may become generalizable in certain directions and standardized for testing purposes. Only representative design can answer this problem. By a set of analyses not reflected in the figures of the present paper, the ecological generalizability of the principle of perceptual compromise, or of "approximate" size constancy as originally suggested by Hering, has been established for our subject along with the broader principle of distal rather than proximal focusing.

Canvassing as accidental quota sampling of the ecology. In still other cases an entire array of individual systematic experiments may appear to be laid out after quasi-representative principles, so as to cover the ecology or the mediational pathways by a vaguely conceived "one of a kind" rule. This tacit sampling procedure forms a counterpart to what polling statisticians might describe as a most rudimentary form of stratified, "quota" or proportionate sampling and is usually of the highly erratic type sometimes labeled "accidental" in statistics. We will speak of this primitive type of coverage of the ecology as "canvassing."

In the field of the perceptual constancies, the attempted extension of basic principles, such as that of distal focusing or of compromise, from size to shape to color constancy, is an example of canvassing. A cross-departmental extension to "loudness constancy" with distance of sound source variant was undertaken by Mohrmann (37), yielding generally similar results. Kinesthetic experiments with falling bodies, with and without the aid of visual cues, have established the functioning of a perceptual "weight constancy," with speed (and thus with kinetic energy) variant (2, pp. 161 f.). The above-cited experiments on value constancy further augment the picture of canvassing of the perceptual constancies.

Truncated factorial design and an impasse in the analysis of variance. The basic intent of representative design is toward design proper; that is, it concerns the employment of a statistical selection device for the stimulus sample; only secondarily is it concerned with statistics as a procedure of evaluation. Its relevance to analysis of variance is thus indirect, and derives solely from the fact that this evaluation technique is an adjunct to factorial design, conceived in its shadow and thus wrought with its inherent inadequacies.

At first glance factorial design may appear ideal, since all plots are filled, while in representative design normally only part of the plots is filled; representative designs would thus seem readily extractable from factorial designs if anyone should wish to extract them, but not vice versa. The first catch is, however, that some of the intercombinations of variates may be incompatible in nature or otherwise grossly unrealistic.

An example is furnished by a study of the perceptual impression values of schematized faces by Brunswik and Reiter (2, 11; for a summary and discussion see 7, pp. 40 f.). In effect, this was an early factorial design, with a $3 \times 3 \times 3 \times 3 \times 3$ layout (Fig. 6). Fortunately for our argument, we were naive enough to depart from the complete array of intercombinations so far as two of the five "facial" variables chosen are concerned. The inadvertently unorthodox part is the truncated treatment of the "nose," as plotted in Fig. 7. Two opposite corners are left empty so that there are only seven noses instead of nine, making for a total of $3 \times 3 \times 3 \times 7 = 189$ facial schemata. The result is an oblique relationship in the artificial ecology of the design which is not unlike the representative ecological correlation shown in Fig. 3 for size constancy, even though with a strange orderly tint that bespeaks the systematic origin of the stimulus distribution.

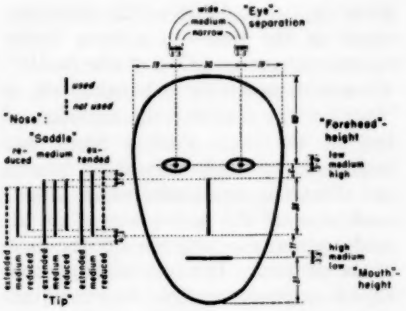


FIG. 6. Five-factorial schematized face (adapted from Brunswik and Reiter, 11).

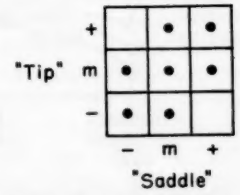


FIG. 7. Truncated treatment of two out of the five variables shown in Fig. 6.

The reason for the departure from strict orderliness of the design was

given by the desire—still somewhat vague at the time—to achieve better representativeness. One of the desiderata was to make the schemata look as "facelike" as feasible; the omission of two of the nine possible noses was prompted by their unusually bizarre and ridiculous appearance which threatened to spoil the seriousness of an already precarious attitude on the part of the subjects. In many other psychological contexts certain intercombinations may be unrealistic or disruptive in similar or in some other ways.

Like the design, our original evaluation was makeshift. Years later the main effects and some of the first-order interactions were found significant in terms of analysis of variance as applied to the 189 original impressions of intelligence (composite from 10 adult subjects), although this of course implies little concerning the ecological generalizability to live faces. For our schemata it appeared that impressions of intelligence are aided primarily by high or medium forehead, medium mouth, and medium nose.

In the analysis of the impression value of the "nose" the omission of the two cells necessitated—as my statistician colleagues, Drs. Rheem F. Jarrett and Robert Rollin, informed me—

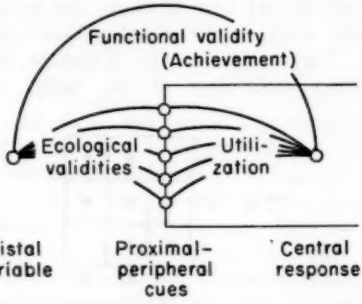


FIG. 8. The lens model as applied to perceptual constancy (adapted from Brunswik, 8).

the employment of such *ad hoc* procedures as the "missing plot" or the "unequal numbers in cells" technique (30, pp. 220 ff.). These techniques are not only artificial and rather uncommon; what is more, they involve the somewhat hypocritical pretense that part of the data was lost, or at least that the material is not quite as it ought to be. Under representative design, involving as it does variate packages in many more dimensions than factorial design could handle practically, oblique distributions are not only legitimate but will in most cases be dictated by nature or by culture, and thus be mandatory. Evaluation techniques will have to be fitted to the materials that may be obtained under such design, rather than vice versa.

MAJOR VARIETIES OF PSYCHOLOGICAL THEORIZING

We now turn to the implications of our above considerations for psychological theory. Three types of psychological theorizing will be discussed: (*a*) theory as the ratiomorphic explication of probabilistic functional—notably distal—achievement and of its strategy; (*b*) theory as the customary nomothetic and more or less molecular reduction of function; and (*c*) theory as comparative methodology, both within psychology and between psychology and the other sciences.

Theory as a ratiomorphic model of functional achievement and its strategy. Focusing and vicarious mediation. The representative study of distal constant function, as reported above, is schematically described in Fig. 8. The wide-arching functional validity coefficient constitutes a generalized statement of the organism's perfection in the attainment of a given distal variable; it falls under the concept of "descriptive theory" by virtue of its generality alone. In addition, an inventory

or mapping of the full array of outpost variables attained by an individual or species, as well as of the degree of such focusing ("psychology in terms of objects," 2, 3), promises to lead to further generalizations concerning preferential focusing on distal vs. proximal variables at large, concerning the relationship between life relevance and distality, and concerning related biological-functional problems.

As is further indicated in Fig. 8, the over-all functional arc may be broken down into an extrasystemic and an intrasystemic constituent, called respectively, ecological validity and utilization. The general pattern of the mediational strategy of the organism is predicated upon the limited ecological validity or trustworthiness of cues which we have observed earlier in this paper. This forces a probabilistic strategy upon the organism. To improve its bet, it must accumulate and combine cues. Thus we arrive at a better understanding of the principle of mutual substitutability, or "vicarious functioning," of means (or cues) for each other which Hunter, Tolman (47, ch. i), and most other behaviorists looking for a structural criterion have incorporated in their basic definitions of behavior or purpose (see 8, ch. ii). Hence the lens-like model in Fig. 8, which may be taken to represent the basic unit of psychological functioning. No matter how much the attainment is improved, however, distal function remains inherently probabilistic.

In the light of this model all "constant," or rather, quasi-constant function, be it "intuitive" or explicit, can be explicated as a statistical reasoning process remindful of Helmholtz's "unconscious inference," albeit without its introspectionistic and perfectionistic overtones. Forming a Latin-Greek hybrid, we may speak of this as a "ratiomorphic" theory of achievement (9).

One of the most important aspects of such a theory concerns the relationship between ecological validity and utilization. Ideally, cues should be utilized in accordance with their validity. But here we must inject, among other things to be brought up later in this paper, the element of "cost" to the organism, just as we must ask for the cost of an automobile along with its efficiency in budgeting our expenditures. Functional theory here takes on certain features of economic theory.

Theory as nomothetic reduction. We have left the discussion of the systematic approach, in favor of that of representative design, at the point where the former was about to embark on a diacritical confrontation of constant function with its own cues or other mediating instrumentalities. This type of confrontation has been practiced extensively in psychology, notably in its more classical phases. In the founding period of behaviorism, Watson and Lashley (50) set out to transport homing birds in closed cages over distances sometimes far outside their natural range. Others have since used anesthetics, faradic cages, or rotation on a phonograph turntable in the dark throughout the journey (45). The artificially distortive laboratory experiments on the perception of size and distance to which we have referred above as preceding, and in part paralleling, the quasi-representative and representative studies of the last quarter century fall in the same category.

The objective of this type of behavior or perception study is to trace performance step by step to identifiable processes, cues, or tracks of mediation; in the end they are to be "reduced" to the "laws" of one of the more microscopic, more "fundamental" disciplines, notably physiology. Traditionally, many

psychologists have seen in such a reductive explanation of behavior the major task of psychological theory. The nomothetic behaviorism of Hull and of his sympathizers, mentioned at the beginning of this paper, reveals its reductionist aim most clearly in the use of a "physiologizing" terminology.

To the study of distal function and of its grand strategy the reductive approach adds the study of tactics; to the study of achievement and of its macromediation, both of which fall within the province of functional-representative design, it adds the study of micromediation, which falls in the province of nomothetic-systematic design. The functional approach has its place mainly in the appraisal of the interplay and relative contribution or weight of factors in the adjustment to a given ecology, while the reductive approach reveals the technological details of the machinery that brings about such adjustment.

Theory as comparative methodology: observational unity and thematic diversity of science. The injection of physiology into the discussion brings us to a branch of psychological theory which overlaps with the philosophy of science and is best labeled comparative methodology of science. One of the major concerns of such a science of science, or metatheory, is with the basic unity of the sciences. In present-day psychological discussion this problem is subsumed mainly under the watchword "operationism." Here it is often forgotten that the basic requirement for scientific exactitude is a relatively modest one, and in a certain sense a more commonplace one than anticipated. It involves no more than the inter- and intrasubjective univocality of observation and communication which is sometimes called "methodological physicalism" (8), but should better be specified as observational or procedural physicalism; but it does not include the univocality of prediction which is the major *raison d'être* of the nomothetic approach (5).

We may therefore take the position that in the end the unity of science is better served by allowing the reaffirmation or elaboration of this unity to be superseded by a working out of the thematic diversity of the sciences within the minimum common platform. This diversity of themata involves both the aim of the different disciplines and the designs capable of serving these aims.

Thematic physicalism and the nomothetic-reductionist-systematic syndrome. The different explicit and implicit trends toward the unification of science which have dominated the last half century have been under the spell of a somewhat stereotyped image of physics. The thematic element in this cliché may be explicated as the emphasis on the "general," and notably on the strict, univocal regularities or laws which possess universal applicability; this is also known as the nomothetic approach. Universality of law presupposes homogeneity of the universe; hence it matters little where and when and over how large an area a phenomenon is studied. Experimental design may thus safely be left to the convenience and liking of the experimenter and thus become systematic. And, as Mises (36) has pointed out in discussing probabilism in physics, macrolaws have their origin in differential equations, that is, in principles conceived for minute space-time splinters. The triad of nomothetic aim, microreductionist procedure, and systematic design which we have come to recognize as a syndrome in traditional psychology thus is revealed as an emulation of a pattern indigenous to the specific thema of physics.

As has been pointed out in greater detail elsewhere (8, sec. 9), the basically elementistic character of physical

law is not obviated by, and has little to do with, the empirical fact of a probabilistic microstructure in thermodynamics or within the atom. Nor must such structures be seen as related to, or in any way supporting, our arguments for the probability approach in psychology; functional psychology is macroprobabilistic in that the identity of the individual case is maintained (as in a scattergram) while it is lost in the physical macrolaw which for ordinary practical purposes is absolute.

General vs. particular. Physical law, geographic fact, ecological correlation. The first to warn against the overestimation of the general over the particular in science was Windelband (52). In a somewhat stilted application to personality and to the humanities, he was led to suggest the well-known distinction between nomothetic and idiographic disciplines. Unfortunately, the latter term encompasses both the low-brow, strictly enumerative approach to historic-geographic fact seen in isolation, and the extremely high-brow emphasis on the "unique" lawfulness of the individual or culture; more properly, the latter case should therefore be labelled the "idionomothetic" approach.

Within the natural sciences, an example of the purely enumerative approach is given by those branches of geography that deal with topographical mapping. The fact that no application of the general physical laws is possible without the constants, parameters and boundary conditions furnished by geographic types of information is frequently neglected in theoretical discussion. Except at the level of control ordinarily accessible only to the physicist observer, these constants are not available (at least not fully so) to the responding organism. Hence the chains from select distal to proximal to central variables in perception are chains of (probable) partial, rather than of total, causation. The universal lawfulness of the world is of limited comfort to the perceiver or behaver not in a position to apply these laws, and he therefore must rely largely on whatever snitches of particular or semigeneralized information he may be able to assemble. This is what we meant earlier in this paper by the assertion that ordinarily organisms must behave as if in a semierratic ecology.

With data from our representative size-constancy survey described above, the relation between physical law and ecological correlation is illustrated in Table 1. The first row of ecological validities has been introduced above in discussing Fig. 3, and the remaining two pairs are added here from our material. Partial correlations derived from either of the two columns of three co-

TABLE 1

TEXTURAL ECOLOGY AND PHYSICS

(Adapted from Brunswik **7**, Fig. 10)

Variables Correlated	Ecological Validities		Nomothetic Approach		
			Partial Correlation		Law of Physical Optics
	Full Sample ($N = 93$)	Sizes over 10 cm. ($N = 59$)	Variable "Held Constant"	Coefficient Obtained	
$B \times P$	.70	.14	D	1.00	$B = PD$
$B \times D$	.77	.88	P	1.00	
$D \times P$	.08	−.34	B	−1.00	

efficients yield (in as close approximation as may be expected) the perfect positive or negative correlations listed in the third column of figures. These in turn reflect the well-known proportionality law, given still further to the right. Since derivation is possible from all alternate ecologies (as exemplified by our sample of 93 vs. the subsample of 59 situations mentioned earlier), it is evident that textural ecology adds valuable probabilistic information to the vastly distilled relational information incorporated in the universal laws of physics. Essentially this is information abstracted and summarized from the geographies that make up the ecology, and is information of the type finite organisms may best be able to absorb in learning, notably in probability discrimination with partial reinforcement.

It is for the same reasons that the laws of triangulation, which underlie the binocular depth mechanism and which only recently have once more been the starting point for nomothetic treatment of this mechanism (**32**; see also **17**), are of somewhat academic interest so far as the actual perceiver is concerned. And for similar reasons we stressed, earlier in this paper, the limited validity of this mechanism within our cultural ecology, in which the perceiver is routinely exposed to optical instruments as well as to flat pictures as substitute means of access to three-dimensional reality.

Nonfunctionalistic (intraorganismic) uses of probability in psychology. It will be remembered that in our molar-functional view of organism-environment interplay, uncertainty is a feature of the relationships between the organism and the distal environment, to wit, of proximal-distal relationships in the case of ecological validity and of central-distal relationships in the case of functional validity; uncertainty is not seen as a necessary feature of intraorganismic processes. In a certain sense this is perhaps in reversal of a tradition which has seen nothing but law in the environment, but was indeterministic or vitalistic so far as the reacting organism is concerned.

Some recent movements have viewed processes within the organism from the probability point of view. While probabilistic functionalism is inherently intersystemic, their approach is intrasystemic. We have no quarrel with them except that we must make it clear that they are confined to problem conceptions which are oversimplified from the standpoint of molar functional psychology, encapsulated as they are within the boundaries of the organism.

Intraregional statistical approaches in psychology may be classified under three major headings: central, peripheral, and peripheral-central. An example of a central statistical theory is the study of "random nervous nets" as developed in the framework of mathematical biophysics by McCulloch and Pitts (**33**), later in collaboration with Landahl (**31**). Concern is not with "this" neurone synapsing on "that" one, but rather with "gross" distribution of tendencies and probabilities associated with points or regions in the net. This statistical "rather than deterministic" biophysics was used by Rashevsky (**39**) as an underpinning for his fundamental equations of mathematical biophysics much in the manner in which the kinetic theory of gases serves as a reductive support to thermodynamics. Rapoport and Shimbel (**38, 42**) have extended the theory to the dynamics of social interaction.

An application of probability to a peripheral problem is given by the "statistical behavioristics" of Miller and Frick (**35**). The exposition is encapsulated within the single region of overt responses. The study of "stochastic"

(i.e., to a certain extent predictable) word sequences in common English which has come in vogue recently is more or less bodily transferred to an analysis of human "courses of action" —which are related to the "strategies" in the playing of games (49)—and of the "dependent probabilities" resulting from the fact that the preceding occurrence of a response does not always return the system to the original state. Here the only cue considered is given by the preceding time series of events of the same kind. This procedure is not indigenous nor particularly congenial to the thematic content of psychology. Rather, it is somewhat mechanically transplanted from those segments of cybernetics in which unidimensional sequential distribution and correlation are predominant (51)—another instance of falling for ready-made gadgets, even though at a more elevated level.

Skinner's (43) concept of "probability of response" may likewise be classified under the heading of peripheral encapsulation, the term being no more than a fanciful expression for (relative) frequency of response as one of the traditional scores in learning experiments involving the motor output.

The "statistical learning theory" of Estes (13) is an example of a peripheral-central theory, or perhaps of a peripheral-peripheral theory crossing through the entire organism from input to output. Based as it is on evidence from systematic experiments, there is reason to doubt that the "behavior samples" and "statistical samples of environmental events" which are linked by the theory are even tacitly envisaged as representative samples.

Rudimentary emergence of the concept of cue in cybernetics and the theory of telecommunication. Outside of psychology, and within movements which in some of their more obvious aspects have become templates for most of the intraorganismic probabilism just cited, a first step in the direction of the bivariate type of correlation analysis which is covered by the concept of ecological validity is made by Wiener (51) in his analysis of double time series. Such series are, as Wiener points out, most conspicuous in economico-sociological and meteorologico-geophysical applications, since in both instances the relative lead of one time series with respect to another may well give much more information concerning the past of the second than of its own. For example, on account of the general eastward movement of the weather, Chicago weather may be more important in the forecasting of Boston weather than Boston weather itself. It will be noted that even here the comparison stays within the same kind or physical denomination ("weather"); in such cases comparisons or correlations are not yet genuinely bivariate as are those between proximal cues and objects, or between means and goal attainments.

A more direct exposition of those mathematical principles of communication which are of particular relevance to the understanding of focusing by vicarious functioning as it occurs in psychological mechanisms has been given by Shannon and Weaver (41). In terms of the vocabulary of the special brand of telecommunication engineering involving semicontrolled media, to which the theory has been geared, perceptual cues and behavioral means are like "signals" in "coded messages." The mediating channels are contaminated with interferences or constraints of their own. The result is equivocation. It is then "not in general possible to reconstruct the message with certainty by any operation on the signal." Shannon's diagram showing the fanning out of "reasonable causes" (messages, inputs) for a given "high probability received signal" or effect, and of "reason-

able effects" (signals, outputs) from a given "high probability message" or cause in a channel, bears formal resemblance to the equivocal types of coupling between intra- and extraorganismic regions to which this writer called attention twenty years ago (2, Fig. 2) and which can also be read into our diagram of the lens model in Fig. 8.

Whenever the "capacity" of a channel is less than the richness of variability of the source from which it accepts messages, the channel is "overloaded." Then it is

> impossible to devise codes which reduce the error frequency as low as one may please. . . . However clever one is with the coding process, it will always be true that after the signal is received there remains some undesirable (noise) uncertainty about what the message was (41, p. 111).

We may add that, in quite the same manner, the crux of organismic adjustment obviously lies in the fact that distal perceptual and behavioral mediation must, by the nature of things, in the general case rely on overloaded channels, with the ensuing limited dependability of all achievement mechanisms. And we may note that at least part of the trouble lies with the overloading and noise in the external rather than the internal medium.

Shannon and Weaver point to one means by which the chances of error can be decreased, however. This is "redundancy," as exemplified by, but by no means restricted to, verbal repetitiveness. When there is noise there is some real advantage in not using a coding process that eliminates all the redundancy, for the remaining redundancy helps combat the uncertainty of transmission. The reader will recognize that the vicarious functioning of cues and of means—functional patterns we came to acknowledge as the backbone of stabilized achievement—may be viewed as special cases of receiving or sending messages through redundant, even though not literally repetitive, channels. The probability of error, given by the variety of possible causes or effects that could result in, or be produced by, the type of event in question, can thus be minimized; this is the case in the gain of the over-all functional validity (.99) over the ecological validity of the major retinal cue (.70) in our representative survey of size constancy referred to above, in which the organism acts as an intuitive statistician.[8]

A suggestion of extending the theory of communication to multivariate patterns of mediation has recently been made by McGill (34). It is hoped that this will open up the full scope of vicarious functioning to formal treatment.

CLINICAL AND RELATED ASPECTS OF REPRESENTATIVE DESIGN

There are several ways in which representative design has become involved with social and clinical psychology. Two of them deal with sampling aspects exclusively, and another two concern both design and the functional theory of the vicarious functioning of cues.

[8] The present statistical application of redundancy is not to be confused with one recently suggested by Attneave (1). In his case the concern is with the exploitation (extrapolation) of *strict* law as it holds over limited stretches of space (or time), say, along part of the contour of an ink bottle. In an earlier context, this writer has spoken of such regularities of limited scope as "local laws" (*Lokalgesetze*, 2, pp. 209, 212). By virtue of their strictness their treatment falls under our above-mentioned heading of an idionomothetic approach; its only relation to the probabilistic approach lies in the fact that the area to which a local law is limited may be considered as a subecology, and that the organism may have to proceed by basically similar mechanisms to ascertain either of the two types of limited regularities, the strict or the probabilistic.

Representative sampling of persons in the role of objects. The case in which not only the responding subjects but also the stimulus objects are persons furnishes perhaps the most obvious demonstration of the necessity for representative design. It is for this symmetry of subject and object that such media as the social perception of personality from photographs seem to offer the best chance of convincing the designer that there must be a sizable *N* of social objects alongside the customary sizable *n* of subjects (judges). Yet a survey of the respective literature revealed that, apparently by force of a somewhat thoughtless, content-bound tradition, the object *N* is on the whole pitifully inadequate in comparison with the subject *n*, with fallacies of generalization ensuing (7, ch. vi and p. 38).

Hammond has been especially astute in exposing comparable lapses in the logic of design and of evaluation in other areas of social psychology (18) and in clinical psychology, notably in the study of the effect of the sex or personality of the examiner upon Rorschach and other projective test results (19)—even on the part of standard texts in psychological statistics. It turned out that it was a surprisingly widespread practice to apply only one (responder-populational) test of significance using *n*, and to tacitly consider this test to cover ecological generality in the same breath.

Representative sampling of stimulus configurations as test situations. Concerning representativeness as to the purely physical stimulus configurations used in testing, test designers have so far not pressed beyond the stage designated as canvassing, earlier in this paper. At best, tests were either picked to reflect roughly the current distribution of systematic laboratory experimentation—in particularly fortunate cases, such as in Thurstone's factorial study of perception (46), with an emphasis on more recent and more complex types of experimental problems—or there was an informal effort to assemble a battery of "close-to-life" situations. If the tests were selected from a pool of existing tests the term "sampling" was occasionally applied, but it was usually forgotten that sampling from an artifact, even when this sampling itself should be random, merely perpetuates earlier bias (7, pp. 50 f.).

Similar considerations apply to the customary so-called sampling of behavior, of acts, or of traits from a trait universe (including the construction of adjective check lists), so long as the respective universes or sampling procedures are but informally scrutinized.[4] The degree of representativeness is in each of these cases determined not by the most but by the least representative step in the chain of defining the universe and of drawing the sample. Since trait sampling concerns the responder rather than the ecology, and hence is not part of representative design proper, this is added here merely parenthetically.

From a technical point of view it is not sufficient to have one close-to-life test, or even many of them. As we have pointed out in the main part of this paper, there must be balanced coverage of life. A mere multitude of situations may be designated as *M* tests, reserving *N* for the size of truly representative ecological samples. (*M* is chosen as the letter preceding *N*, or as the first letter in *many;* the capital letter is used in slight departure from the practice suggested earlier [7, pp. 34 f.]). Only for a true *N* can technical tests of ecological significance as envisaged in representative design be applied.

[4] Guttman states, seemingly without being much concerned about the underlying problem, that "the processes of sampling people and of sampling items are not at all identical; random sampling, stratified or not, is used for the first, but is not applicable to item construction" (44, p. 54).

Such stimulus configurations as the Rorschach inkblots are biased variate packages, in the sense of the word as defined earlier in this paper. They invite what Sander (40) has called "realistically-meaningful" (*sinnhaft-bedeutungsvoll*) or "ontotropic" tendencies, in contradistinction to the "geometric-ornamental" or "eidotropic" tendencies (toward *Prägnanz* or "good form," such as abstract circles, squares, or other purely geometric formations) as described by the gestalt psychologists proper. Since these ontotropic tendencies are considered to represent a higher developmental stage than the formalistic eidotropic tendencies, both in ontogenesis and in the "actualgenesis" of the instantaneous perceptual experience, a more genuine sampling of stimulus patterns and the setting up of more representative evaluation categories may become diagnostically valuable.

Allowance of vicarious functioning as a representative feature in test construction. As we have seen, one of the most important principles of functional theory is that of vicarious functioning. Its earliest recognition stems not from academic psychology but from one of the theoretical antecedents of clinical psychology, psychoanalysis. Under the direct influence of psychoanalysis, Frenkel-Brunswik (15) has developed the study of "alternative manifestations" of the latent dynamic structure of needs on a statistical basis, using multiple correlation with overt behavior elements; the way to the rational reconstruction of clinical intuition which is asked for in another line of Vienna tradition, logical empiricism, was thus opened. The relationships that may or may not exist with Lazarsfeld's subsequent theory of "latent structure" (44) will not be discussed here.

In my own work the attention to vicarious functioning has concentrated on perception rather than on behavioral expression (and, within perception, on physical objects or on the more static traits of social objects), and the implications of vicariousness upon representative design have been developed through this medium (7, ch. v, vi, viii; for more general discussion see 8, ch. ii). In his contribution to the present symposium Hammond (20) has expanded this to demand allowance of multiple mediation patterns for, and an attendant multiple cue analysis of, clinical judgment by recourse to the requirements of representative design.

Ecological validity and utilization of clinical cues. A problem ensuing from the work of Frenkel-Brunswik and of Hammond, just cited, is the question about the extent to which the ecological validity of the potential cues available to clinical intuition is duplicated by the weight given these cues in their utilization on the part of the responding clinician. Hammond and his collaborators at Colorado (20), and, independently, Smedslund at Oslo (in an as yet unpublished study), have recently found cases of gross discrepancy in this respect, involving both the ignoring or even the reversal of valid cues and the overutilization of cues of low validity.

Pitfalls of systematic design in biological application. As we have seen, representative design is especially indispensable whenever the relative contribution of different variables in a functional context is the subject of investigation. But it must not be forgotten that any systematic experiment, regardless of how oddly conceived it may be, represents at least one actual or potential ecological instance, and in this sense is a bit of reality; in addition, as we have seen, it may be the only means of obtaining knowledge in the reductive context of science.

Impressive as are the achievements of reductive experimentation in the biological sciences, the dangers resulting

from the tying of variables and from other characteristics of nonrepresentative designs have rarely been completely avoided. The history of science furnishes ample evidence of harmful effects of systematic design upon practice or theoretical outlook. As a layman, one may think of hygiene in medicine with its dramatic changes between the complete neglect and the excesses of aseptic or antiseptic policies; the boiling of milk and devitaminization; anemia and the eating of liver, and so forth. All of these practices appear as playballs of variables arbitrarily selected for study, which thus acquire undue prominence and throw the picture of the interplay of factors out of balance; and the shortcuts involved in the ecological overgeneralization of results may be even more serious. The biochemist or nutritionist who shuns eggs but is a chain smoker, and the cancer specialist who does the opposite, are too familiar examples to require elaboration.

Systematic design in cliché literature. We may also regard the "world" of popular novels and movies as an artificial, cultural subecology. Its outstanding feature is the presence of clichés; these include both personality stereotypes and plot formulas. Clichés are similar to experiments using systematic design. Factors that show some degree of independent variability in real life are artificially tied. Old-fashioned opera plots or soap melodramas that "drip with generosity" share with cowboy movies what David Hume would have called "inseparable" associations of noble character, overpowering strength, fairness, courage, youth, final success, and so forth, or of their opposites. The cliché is a "worn" case or incident, by no means impossible or nonexistent but made prominent out of all proportion to its frequency, and to the detriment of all other types of incident. Like the systematic design of experiments, it superimposes artificial "laws" upon an ecology which it thereby depletes.

In the case of the systematic experiment it is these artificial laws in the design which, as we have suggested, are at least in part responsible for the often striking lawfulness of the results. At this point the suspicion arises that the didactic role which systematic experimentation obviously plays in the mental economy of the scientist, by virtue of the simplicity and order it both requires in the design and furnishes in the result, may outweigh the fact-finding competence of systematically designed experiments. Certainly the more drastically simplified forms of art of which we have spoken, and which are so similar in pattern to the systematic experiment, are clearly didactic rather than informative in any realistic sense; we do not go to the movies to find out about life or to form a scientifically airtight theory about personality, in spite of the fact that movies may sometimes be helpful in temporarily smoothing the perplexities of life.

The main function both of art and of systematic experimentation, then, is to shake and mold us by exaggeration and extreme correlation or absence of correlation. But exaggeration is distortion, and this distortion must in science eventually be resolved by allowing the more palatable systematic design to mature into, and to be superseded by, the more truthful representative design.

CONCLUSION: UNITY OF SCIENCE AND UNITY OF PSYCHOLOGY

Our considerations in comparative methodology have brought into focus the thematic diversification that is possible within the over-all unity of exact, observationally physicalistic science. They may facilitate the cheerful relinquishment of the overheated nomothetic bias under which the development of psychology has long suffered and

which is making the establishment of a molar-functional psychology an uphill battle. Acceptance of the probabilistic conception, both for the propaedeutic study of ecology and for the functional analysis of perceptual or behavioral achievement, not only sets normative psychology on the right track; it also unites it methodologically with differential psychology, thus lending ideological support to the inherently statistical character they both share, and making a virtue of this often bemoaned necessity.

Psychology thus acquires the distinctive, well-circumscribed internal unity and coherence which we have long searched for, and the reality of which many of us have doubted. This unity emerges, as does perhaps all good unity of—and in—science, as one of methodology or grand design. In both of its major aspects, representative design and functional theory, it centers about probabilistic texture and the ways of its exploitation by the organism. The fact that organismic achievement and its reconstruction in psychology are characterized by certain similarities with predictive procedures in economics, meteorology, and especially in the study of telecommunication through uncontrolled, noisy external media, should help to bring into focus the family of sciences in its entirety. Such a focusing should help to set in proper perspective our traditional thematic dependence on, or need for succorance from, a select few of the older disciplines which may have appeared particularly glamorous, especially physiology and physics.

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THE PROBABILITY APPROACH AND NOMOTHETIC THEORY

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Brunswik has made an eloquent case for the formulation of psychological laws in terms of probability linkages between environmental events and organismic achievements (1, 2, 4). There are three main parts to his thesis. First, certain assumptions are made concerning the present and future aims of psychological description and theory; second, the inadequacies of the nomothetic or "uniformity" point of view for attaining these objectives are pointed out; third, the constructive program of the probability approach and the associated method of representative design are presented. In this paper, we shall consider each of the opposed positions, in turn. The strengths and weaknesses of the probability approach and of the nomothetic point of view will be juxtaposed. While recognizing the cogency of many of Brunswik's criticisms, we shall argue for a reaffirmation of the nomothetic position.

Focus on Conditions Representative of Real Life

The aim of psychology, from the probability point of view, is to throw light on behavior as it occurs under natural conditions. "Psychology has to focus its descriptions on what the organisms have become focused on" (4, p. 136). The order of ideas must be the same as the order of things. Many psychologists today would probably be willing to subscribe to this programmatic statement; research should approach real-life conditions as closely as possible. But perhaps they would be tempted to subscribe to this objective more readily than they should. It is not necessarily true that investigations are most profitably carried out under the very conditions which they are intended to explain. We are all familiar with the well-worn examples of engineering problems being solved through theoretical physics and pure mathematics. Such, to be sure, is not always the course of events. Many applied problems can be solved only in applied contexts. Generalizations from restricted laboratory findings to specific life situations must always remain precarious. There are, however, great advantages in entering the concrete situation armed with conclusions obtained under controlled, even though artificial, conditions. As Hilgard has suggested, "*A principle once discovered in a better controlled situation can be validated in a less well controlled one.* Even though the sole aim of learning experimentation were practical, it might prove economical to work in the laboratory in order to find the leads worth testing in practice" (7, pp. 358 f.).

Even if we were to agree, then, that the purpose of psychology is to systematize our knowledge of behavior as it occurs in the organism's habitat, it does not follow that the analysis and explanation of such behavior can be pursued only under "natural" conditions. In behavior analysis, as in problem solving, the *detour principle* may provide the optimal road to solution. By turning our backs on the complexities of behavior in the organism's natural habitat we may arrive more speedily at useful tools of analysis than if we allow ourselves to be guided by the apparent lack of uniformity in daily behavior.

From the probability point of view,

laboratory situations artificially produce correlations among variables which are higher than those found under natural conditions. Granted that the stimulus-response correlations as we observe them daily typically fall short of unity, the explanatory premises from which we would hope to derive these facts of behavior need not themselves have the form of contingency or probability statements. To quote Feigl, explanation may be taken primarily to be a *procedure of inference*, and "the necessity which is bestowed upon the facts by their explanation is the *logical necessity* of the *implication* underlying the inference from assumptions to conclusions" (5, pp. 284 f.). Stimulus-response correlations of less than unity should be derivable from explanatory premises which are stated as universals. And the validity of the explanatory premises may usefully be tested in restricted laboratory situations in which artificially high correlations are produced.

EMPHASIS ON ACHIEVEMENT

Closely related to the focus on real-life situations is the emphasis which the probability point of view puts on organismic achievement. The responses of the organism are described and measured in terms of their "functional validity," i.e., with reference to the organism's ability to attain knowledge about the objective environment, "to reach out cognitively into the farther surroundings" (1, p. 135). Thus, the correlation between physical size and perceived size gauges the functional validity of the size judgment, i.e., measures the degree to which the size perception represents a successful adjustment to the environment. Interest in organismic achievement makes correlational analysis the paramount quantitative method of probability psychology. Correlation coefficients measure the extent to which environmental contingencies are properly evaluated by the organism.

"Achievement" and "functional validity" are terms which denote an *evaluation* of the organism's performance. With such evaluations we cannot, of course, quarrel. We may, however, raise the question whether such normative considerations should mold the description and analysis of psychological facts. Should such evaluations not come more properly *after* psychological description and analysis? And they may, of course, implicitly precede such description, inasmuch as they influence the investigator in the choice of his problems. The analysis of behavior gains in flexibility and generality if it is made without reference to adjustive value.

Let me try to illustrate this point with reference to perception, where emphasis on functional validity may result in limitations on the scope of inquiry. In the past few years, there has been considerable interest in the relation between motivation and perception. Experimental evidence has been offered in support of the hypothesis that motivational factors may significantly influence perceptual discrimination. If true, such facts should hardly be startling. Perceptual discrimination is, after all, behavior, and the study of motivational determinants of behavior is one of the major concerns of contemporary psychology. Nevertheless this work, quite apart from the many experimental uncertainties, has given rise to theoretical controversy quite out of proportion to the magnitude of the empirical results. The core of the difficulty lies, I believe, in explicit or implicit assumptions about the functional validity of perception. The function of perception, most of us firmly believe, is to give correct information about the distal environment. Hence, we expect perceptual theory to

explain the ways in which such achievements are realized.

Now data are presented indicating that in some situations there may be systematic biases operating against functional validity. Neither a classical (psychophysical) theory nor a probability theory which is built around the principle of functional validity can readily assimilate such data. On the part of psychophysical theorists there has been a strong tendency to treat such findings, almost by fiat, as nonperceptual, e.g., by assuming that motivation may affect judgment or report but not perception. For example, Gibson (6) attempts to draw a systematic line between "perception" and "misperception," and Pratt (12) argues that only discriminations which show a univocal correlation with dimensions of the physical stimulus should be considered perceptual. Strong opposition has been expressed to the mere possibility that perceptions may be determined by so-called internal conditions of the organism as well as by external stimulus patterns. As might have been expected, the specter of solipsistic encapsulation has been raised (8). The answer to all this is, of course, that we must not prejudge the facts. Motivated perceptual selectivity either does or does not occur. To the extent that it occurs, it is a function of specific and restricted antecedent conditions. How well perceptual discriminations correspond in any given situation to other measurements of the stimulus, i.e., its distal properties, is a further empirical question.

The probability point of view, to be sure, stresses the necessary imperfections of perceptual achievement, due both to the ambiguities in the causal texture of the environment and the limited capacity of the organism to utilize available cues. Variations in motivation would be viewed as contributing to the imperfections of achievement. However, since the problems of perception are formulated primarily in terms of correlations between the distal properties of objects and the responses of the organism, motivational variables are a source of "noise" interfering with the attainment of veridical information. The specific conditions under which such variables are effective and the extent of their contribution do not become the focus of experimental inquiry, because achievement-centered observations are summed over a wide range of motivational conditions.

I have dealt with the motivation-perception problem at some length because it illustrates the restriction upon analysis and the loss of information which we risk when we use achievement as a point of reference for the formulation of our problems.

THE PROBLEMS OF MEDIATION

The uniformity point of view is criticized on the grounds that it concerns itself prematurely and unsuccessfully with mediational processes, i.e., with the specific mechanisms intervening between stimulus and response. There is a tie-up between the nomothetic and microscopic attitudes. As Brunswik says, these associated interests in uniformity and mediation lead to concern with "artificially isolated proximal or peripheral technicalities of mediation" (1, p. 262). From an achievement point of view, principles of mediation established under such conditions are vitiated by the facts of vicarious function: the same achievements can be brought about by a variety of different mechanisms.

The first question I would like to raise is whether there is indeed a necessary connection between the nomothetic position and focus on mediational processes in general, and molecular or microscopic processes in particular. The

essence of the nomothetic position is that it looks for uniform laws of behavior, e.g., for stimulus-response correlations which are unity, or, at least, theoretically conceived to be unity. The search for such empirical laws can proceed, at a descriptive level, without concern for mediating processes of any kind, whether molar or molecular. In fact, some of those psychologists who have taken an explicit position against mediational hypotheses are equally firmly in the nomothetic camp. Skinner, for example (even though he defines learning as a change in the probability of response), is a nomothetic theorist who vigorously denies the usefulness of mediational hypotheses. He writes: "A science of behavior must eventually deal with behavior in its relation to certain manipulable variables. Theories—whether neural, mental, or conceptual—talk about intervening steps in these relationships. But instead of prompting us to search for and explore relevant variables, they frequently have quite the opposite effect" (13, p. 194), viz., to create a false sense of security. I do not agree with Skinner, but merely cite him to support the point that the search for nomothetic laws does not necessarily entail a commitment to search for microscopic mediational processes. And certainly the advocates of "dimensional analysis," such as McGeoch (10) and Melton (11), combine a nomothetic with a nontheoretical approach. They have been guided in their experimental research by the dimensions of variation in terms of which learning experiments can be described and measured. Their generalizations take the form of precise, quantitative statements about the dependence of learning on a multitude of experimental conditions. At the same time, they have avoided problems of mediation and have not ventured beyond the assertion of empirical laws.

When nomothetic investigators have gone beyond the statement of empirical laws and have invoked intervening variables, they have not necessarily turned to problems of mediation. Rather, the primary use of intervening variables seems to have been to help in the formulation of empirical laws. In first proposing the use of intervening variables, Tolman made it clear that such constructs were to be used in the specification of empirical relationships which, because of their complexity and because of the multitude of variables involved, defied any simple statement. The function relating behavior to antecedent conditions, he wrote, "is a very complicated function . . . it is in fact so complicated that we at present seem unable to state it in any single simple statement. We find, rather, that we have to handle it by conceiving it as broken down into successive sets of component functions" (16, p. 91). At the same time Tolman was careful to distinguish such operational behaviorism from what he called physiological behaviorism, in which the intervening variables would refer to certain peripheral and central disturbances in the nervous system. Spence takes essentially the same position. Theories employing intervening variables "serve primarily as a device to aid in the formulation of the empirical laws. They consist in guesses as to how the uncontrolled or unknown factors in the system under study are related to the experimentally-known variables" (14, p. 72). In short, many nomothetic theorists, and indeed the most influential ones, have deliberately deferred the problems of microscopic mediation. The behavioral scope of such theories depends on the choice of stimulus and response units to which the intervening variables are anchored. Clearly, the larger the units chosen the less precise will be the predictions which can be made by means of the intervening vari-

ables. In that respect, the nomothetic view and the probability view are in the same boat. The difference between them lies in the reasons postulated for the failures of prediction. Proponents of the nomothetic point of view see the difficulty in inadequate knowledge of antecedent conditions, while retaining their belief in the essential uniformity of the behavioral laws (9); proponents of the probability view believe that the laws themselves must be stated in probability terms.

I would conclude that there is no necessary connection between the nomothetic position and focus on microscopic problems of mediation. The nomothetic and probability views do not need to differ with respect to the scope of behavioral problems to which they address themselves. These two points of view do, however, clearly lead to different types of experimental design, and therein I see their most crucial and practically important difference. The problem of representative design which is generated by the probability point of view will be considered next.

REPRESENTATIVE DESIGN

On the methodological side, the probability point of view calls for substitution of representative design for the systematic design which has become traditional in psychology (1). Systematic design is distinguished (*a*) by the orderly manipulation of a limited set of variables, with other variables held constant and (*b*) through practical necessity, by the use of only a restricted range of values of the variables manipulated. Both of these features are criticized as unrepresentative of the conditions which the organism encounters in its natural habitat. Systematic control is said to tie together artificially or to untie variables in ways which violate their natural concomitance. Furthermore, when only a restricted range of values is used, generalizations concerning the effects of a variable become precarious. In the light of these criticisms, representative design calls for the representative sampling of situations or objects as well as subjects. For such a sample, correlations can be determined between distal and/or proximal stimulus characteristics on the one hand, and responses or achievements of the organism on the other. The use of this design necessarily entails the abandonment of classical notions of control; we must take the interaction among variables as we find it in the situations included in the sample. There is, so to speak, controlled noncontrol of the variables.

We must admit that the systematically controlled experiment allows of only limited generalization concerning the effects of the variables manipulated. We know this particularly to be true with regard to motivational variables in human experiments where the experimenter, for practical and ethical reasons, can manipulate only a small fraction of the range of motivational intensities encountered in real-life situations. To a less obvious but probably equally serious degree the same limitations apply to some of the major parameters of learning, such as frequency and the pattern of rewards and punishments. In the face of these difficulties, the value of systematic experimentation can be maintained for several reasons. (*a*) The systematic experiment serves to determine the basic functional relationship between the variables studied, i.e., the shape of the function, which may then be extrapolated to values of the variables not measured in the experiment. If the extrapolation is unsuccessful, further experimentation is indicated. (*b*) The value of systematic experiments may lie not so much in establishing the relationship between the total ranges of two or more variables as in

testing the implication of a theoretical proposition. It seems that a large majority of current experiments in the area of learning is conceived for this purpose, often to arbitrate between what are believed to be contradictory implications of the views of Hull and Tolman. The spate of experiments on latent learning is a good example. (*c*) The systematic experimenter may be content for the time being to restrict his generalizations to the conditions studied, to build "miniature systems" preliminary to the attack on the large-scale problems. By and large, the proponents of so-called "dimensional analysis" have adopted this position.

Let us next consider the advantages and disadvantages of representative design in meeting these shortcomings of systematic experiments. That representative design is experimentally fruitful has been abundantly proven by the ingenious studies of Brunswik and his students. Especially noteworthy is the study of "distal focusing in perception" (2), in which a sample of size-judgment situations was obtained in the course of the subject's daily routine. This experiment served to give the principle of size constancy wide situational generality. Perceptual experiments such as these are peculiarly favorable to the application of representative design. With respect to other problems, very serious difficulties may arise.

Clearly, we can proceed with representative sampling only if we have (*a*) knowledge of what the universe to be sampled is, particularly what the units are of which it is composed, and (*b*) if we have identified the dimensions or features with respect to which the sampling is to be representative. In an experiment on size constancy, these requirements can be met without too much difficulty. The universe to be sampled consists of size judgments of specific objects at specific moments in time. The parameters with respect to which we would want the sample to be representative are, among others, the physical and retinal sizes and distances of the objects. It is these variables which have entered into the computation of the correlation coefficients, in the study already cited, to determine the functional validity of the size judgments.

We are confronted with very serious difficulties, however, when we contemplate extension of such a design to other areas of behavior. Take learning as an example. If we were to embark on the enterprise of obtaining a representative sample of learning behaviors, the first question we would face would be what a unit of learning is. How do we identify an instance of learning when we encounter it? I suppose we could arrive at some criteria, but these would inevitably reflect considerations derived from nomothetic theory. We could, for example, look for the appearance of new motor movements and identify each of these as a unit of learning. Or we could define units of learning as new-means-end relationships, in Tolman's sense (15). Our choice of unit will critically affect the nature of the sample we would obtain, to say nothing of the correlations between stimuli, responses, and environmental effects. I am wondering, therefore, whether it is possible to carry out representative designs which are not, in fact, large-scale nomothetic experiments, except for wider sampling of interactions among variables. Such at least appears to be the case when we go beyond the perceptual paradigm, but upon further reflection we may even want to include the perceptual studies under this statement. For the choice of the variables of physical size, retinal size, and judged size implies nomothetic concepts concerning the conditions of size discrimination.

A final word concerning the noncon-

trol of variables in representative design experiments. The fundamental reason for eliminating systematic controls is, of course, the desire not to interfere with the free play of variables as it occurs naturally. One consequence of this approach is that we may deliberately forego the possibility of measuring precisely the nature and amount of interaction among any given limited set of variables. For example, if color is allowed to vary at random when we sample a variety of physical sizes, it may not be possible to ascertain the exact nature of the interaction between physical size and color in determining perceived size. This is as it should be if we are concerned only with the cue value of physical size over the total range of environmental conditions. We are, however, running a calculated risk of sacrificing potentially valuable information. Suppose it were true that differences in color are important when the physical size is small but make no difference when the physical size is large. Since the color variable would not have been systematically controlled, the combinations of sizes and colors necessary for establishing this interaction may not be present in our sample of objects. Moreover, the over-all correlation between color and size judgments would be low, even though it might be high over a restricted range of physical sizes, and low over the rest. Since the nature and amount of interaction among variables are not measured, it would be difficult to apply the findings of representative design experiments to the prediction or control of specific situations.

CONCLUSION

I would conclude, then, that nomothetic theory can meet the very important and serious criticisms advanced by the probability position along several lines: (*a*) some of the fundamental assumptions of the probability position, particularly the focus on real-life situations and achievement, are open to debate; (*b*) the multiple causation of behavior and uncertainty of mediational processes do not preclude the usefulness of explanatory premises asserting universals; (*c*) both systematic and representative design have a price to pay in terms of sacrifice of information—systematic design by artificially isolating, tying, and untying variables; representative design by foregoing the measurement of the interaction of variables in specific situations. Moreover, representative design in the last analysis must start with nomothetic hypotheses in the selection of variables to be sampled and correlated.

In summary, then, I would like to put in a plea for the *status quo*. However, there is room for peaceful coexistence of the two approaches. The search for nomothetic laws of behavior cannot help but benefit from the probabilist's reminder that behavior does not begin nor end under the controlled conditions of the experimental laboratory.

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DISCUSSION OF PROBABILISTIC FUNCTIONALISM

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We all owe a debt to Brunswik (2) for insisting that we should sample the environment as well as the individual if we are to develop a satisfactory science of human behavior. Let me first of all add an example to show that we need to test our generalizations in the widest range of ecologies.

The example is from some unpublished work of Sears and Whiting (4) concerned with the effect of child-rearing practices on personality. Sears set out to study the later consequences of the time of weaning, in line with the Freudian conjecture that weaning is frustrating, and so, if attempted too early, results in later disturbances of behavior. Sears found, contrary to the Freudian hypothesis, that the *earlier* the child was weaned, the less he showed certain types of disturbed or neurotic behavior in later childhood. The relationship was systematic enough to show a degree of lawfulness, the least neurotic children being those who had been fed from a cup at birth, and the amount of disturbance increasing with the postponement of weaning. I am making no effort to describe the study in detail, for I can make my point without more precise specification.

Now Whiting, following the same line of conjecture that Sears followed, and working as an anthropologist together with Sears in the same Institute, assembled data on a wide range of nonliterate cultures. He, too, was able to show a relationship between age of weaning and later disturbed behavior of the kind agreed upon between him and Sears and of the same sort that Sears had studied. But the relationship that he found, while also lawful, was the inverse of that which Sears found: he found that the *later* the weaning, the less disturbed the later behavior. Here is a superficial contradiction. It turned out that the range of weaning ages in our culture is much more restricted than that in primitive cultures. The late weaning studied by Sears was at about a year of age, while the early weaning studied by Whiting was at that age, and the late weaning for Whiting came at three or four years of age. Where the two populations, the nonliterate population and the Western culture, overlapped in weaning age, the amount of neurotic behavior in both studies agreed. I hope that I have made my point clear. The contradiction between the Sears and Whiting findings reduces to a difference in their ecological sampling of weaning ages. I give this illustration in support of Brunswik's caution that we may get into trouble unless we sample the whole range.

I would not go the further step, however, and say that Brunswik's wise suggestions force us to the rejection of the nomothetic approach. I believe that we can incorporate his suggestions without revolutionizing our logic of experimentation.

Let me put this another way. When we talk about scientific method we often lose sight of the substantive problems that the practicing scientist faces. The aim of science is, after all, a rather simple one: to yield new information about things and events and relationships, information that is communicable, verifiable, reasonably economical, and relevant to our needs. Hence the scientist, in order to meet these require-

ments, must have a feel for his subject matter; he must work in an appropriate scale of time and space. If he is measuring tree trunks, he doesn't use micrometers, and if he's studying the gratings of crystals, he doesn't use yardsticks. Now, the use of a micrometer and a yardstick is logically the same, but the scientist has to adapt his tools to his problems. There are no guarantees that the scientist will do good work simply because he uses one scientific logic or another. His critical methods must be adapted to the pitfalls of his particular kind of experimentation.

If nomothetic, test-tube experiments have their problems, as indicated by Professor Brunswik, so have experiments with representative designs. Some of these difficulties were well described by Postman (3), and I will not review them. I would call attention to a problem that arises in connection with the correlational study of size constancy, as used in Brunswik's investigations (1); this is to point out that representative designs are no more foolproof than the other types of systematic design. In his experiment, the subject was asked to estimate the size of various objects in a natural setting—trees, telephone poles, barns, bookcases, inkwells, objects of a wide range of size and at a wide range of distance. The estimates were rather accurate, as represented by correlation coefficients between physical size (as measured at the surface of the object) and the estimates by the subject. Yet, such high correlations could be obtained with this range of data even with many errors in detail; some errors were as great as 3:1, that is, some object might be reported as 3 times its object size, or ⅓ its object size without distorting the correlation in the .90's.

Suppose, now, the experiment were repeated with a blindfolded subject. He would score pretty high, too, by assigning a size appropriate to the name of the object, that is, a house size, an inkwell size, a book size, etc. His estimates too, like those of the subject in Brunswik's experiment, would be much nearer to object size—that is the distal size—than to proximal photographic size. Of course, he would not have a basis for estimating proximal size, for he would not be told the distance of the object, or even if he would be told the distance of the object, he would have no way of using it. In other words, the main conclusions from Brunswik's research in the perceived size constancy would be obtained with a subject who could not perceive because his eyes were bandaged. This follows because in this experiment there is an artificial tying between known size and perceived size. This kind of error could not occur in the laboratory study of size constancy.

I am not seriously suggesting that Professor Brunswik should have used a blindfolded subject as a control in his experiment, yet the suggestion is not actually farfetched. I am making the point only that critical experimentation is always difficult, and no general methodological solution will take care of all contingencies.

I had the feeling, in listening to the papers, that there may be some confusion in overemphasizing the contrast between nomothetic and probabilistic methods of science, in view of the fact that this contrast is found only in selected illustrations. The contrast is very slight, for example, in the biological study of genetics where a definitely nomothetic gene theory works hand in hand with the statistics of Mendelian theory, say in population genetics. Similarly, some of the solutions of problems in thermodynamics are definitely probabilistic without seriously conflicting with nomothetic laws of energy transformation. Therefore, a nomothetic

approach does not require a single track or a microscopic explanation; I think this point has already been made by Postman. And the matter of uncertainty: surely uncertainty does not arise just because of the uncertainty of the environment but also in many other ways, so that the distinction between lawfulness and statistics is not unique to the distinction between systematic experimentation and representative design.

Now the matter of working backwards from after-analysis. This bothers me in view of certain very serious problems of partial correlational analysis which I am not going into; but let me simply say that many of the analyses we have had of the nature-nurture controversy by means of correlational analysis lead me to suggest that correlation is an instrument of the devil. One has to be *extremely* careful in making any kind of analysis into scientific laws on the basis of correlational analysis unless one already knows the causal determiners which are, in a sense, nomothetic. That is, if you know the nomothetic analysis, then you can specify sizes, etc., in terms of correlation.

And then I wonder also if the use of aftereffects would have worked, say, in the fields of bacteriology or dietetics which Professor Brunswik has mentioned in passing, as he spoke about enriched bread, or chain smokers. I wonder, for example, if a study of recovery from pneumonia would ever have found that those people who recover more rapidly are those who happen to have taken penicillin; or that the correlation between diabetes and insulin would have ever resulted just from a very careful correlational study of diabetes and its recovery without some kind of experimental intrusion in the way of systematic design. It is very easy to pick examples on one side or the other; perhaps it's unfair to do so, but if you can pick examples on both sides then the question of which method is better seems to me a practical one rather than one of basic theory.

And thus I find myself led to a conclusion rather like Postman's, in defense of the *status quo*. Perhaps I can paraphrase my attitude toward the nomothetic-probabilistic design problem in this way: I should wish to be as nomothetic as I can be, while as probabilistic as I have to be. If we were to accept this solution, I think we would have learned the major lessons to be taught us by Brunswik.

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DISCUSSION: THEORY AND REDUCTIONISM

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I feel that the differences among us are primarily differences in preferences and taste, or perhaps of practice, or of dogma. Brunswik, to use a cliché of Einstein, thinks that "God *does* gamble." I think He does not. Neither of us has any access to God, neither of us knows, and so it's just a matter of preference. The difference between Postman's position and my position is that he does not want to bite off more than he can chew, and I want to bite off everything in one big bite, and that again is a difference of taste.

In the face of Brunswik's persuasive arguments (1), Postman (6) has, in my opinion, countered each one with an equally persuasive counterargument. Point for point (except for one or two rather important ones) I would like to associate myself with those counterarguments. My most general (and perhaps least fair) reaction to Brunswik's position may be summarized by saying that he has taken a methodological *criticism* and made of it a *cosmology*.

I agree with him when he says, "The chief objection against any systematic design and the laws derived from it is that these entail definite dangers of interpretation." Of course they do! And we should be eternally grateful to Brunswik for alerting us to some of the dangers and misinterpretations. But interpretations have been, are, and always will be dangerous—I might say, as satisfying as they are dangerous. Just because there exist dangers of interpretation in the seeking after nomothetic laws is no argument at all for intellectual resignation. We must avoid throwing out the baby with the dirty bath water—even though the experimental water be as murky as Brunswik believes and even though our psychological baby be as mewling and puking as our critics assert. And besides—some of us prefer to interpret dangerously rather than to correlate safely.

But of course Brunswik's championing of probabilistic theory does not derive only from his fears of "definite dangers" of interpretation. He asserts and reasserts that *all relations in psychology remain probabilistic in principle*. And here he loses me completely —or almost completely. Later I shall indicate a possible reinterpretation of Brunswik's dictum with which I have some sympathy.

I will agree that thus far we have no one-to-one relationship in psychology. I will agree that correlations of less than unity can be useful. I will even agree with Brunswik's very acute observation that psychology seems to become more and more probabilistic as it becomes more and more molar. But nevertheless I remain singularly unimpressed with his plea that we must go the whole hog and assert a probabilistic theory. The reasons for my recalcitrance are fundamentally matters of preference and faith. I *prefer* nomothetic laws. I have faith (with Einstein) that unambiguous regularities will be discovered. Let me, therefore, spell out my intuitions (rather than reasons) for disagreeing with Brunswik. And in so doing I think that I can take on Postman at the same time.

Postman, in countering Brunswik's charge that the uniformity point of view concerns itself with *mediational* processes, argued that Brunswik's charge simply was not true. That is, Postman

maintained the thesis that there is no necessary tie between the nomothetic and the microscopic attitudes. Here is where I believe Postman nodded, and here is where I believe that Brunswik has the better of the argument. I would not only confess to Brunswik's charge, but I would loudly assert, with complete lack of shame, that this tie is the crowning glory and wonderful promise of the uniformity point of view. In other words, Postman denies a virtue. Postman, in denying this grace, takes a Brunswikian achievement point of view and argues that constructs in psychology are useful only to the extent that they help us in the description and prediction of behavior. And then Postman attacks the neurologizing psychologists because of their unbusinesslike behavior of being too free with promissory notes by quoting Kessen and Kimble's (3) criticism of my own speculations (4) to the effect that "the mere claim that a theory is neurological does precisely nothing to give it greater authority than a purely psychological approach."

And here is what I would have to say to both Brunswik and Postman (remember—I am spelling out my preferences, faiths, and intuitions):

a. I have always made it a cardinal principle to live beyond my income. And although I have yet to find a one-to-one correlation in psychology, Professor Brunswik—or between psychology and neurology, Professor Postman—I am always ready to make another promissory note and promise that if you bear with us we *will* find uniform laws, Professor Brunswik, and we *will* find a neurological basis for behavior, Professor Postman. And if I can't pay off on my first promissory note I will come seeking refinancing. For deficit financing, it seems to me, is a sound economic principle—it certainly is a sound economic principle for any enterprise based on faith; and certainly science and the intellectual enterprise is that. I have faith that despite our repeated and inglorious failures we will someday come to a theory which is able to give a consistent and complete description of reality. But in the meantime, I repeat, you must bear with us.[1]

b. My second point boils down to a simple "Down with Prediction!" Of course we all want to predict. As a scientist I want to encompass the universe. That is, no matter how useful probability statements may be, and no matter how useful one-to-one correlations between stimulus and response may be, I still feel a lack of completion in a correlation coefficient of +1. Even if a hypothetical construct *does not add one whit* to my predictive power, I would feel much happier for having that construct if it promises to bring into the matrix of correlations *one more observable event*, useless though it may be for the psychologist *qua* psychologist.

c. My third point gives me an opportunity to agree with Brunswik (although on my own terms), and I think that is the proper way to end my comments. I am convinced that it is in the microscopic world that one will most readily find uniform laws. And so Brunswik is eminently right when he argues that psychology becomes more and more probabilistic as it becomes more and more molar. In that sense I understand and agree with Brunswik's dictum that all relations in "psychol-

[1] As I ready these informal comments for publication I cannot refrain from pointing to our recent article on enzyme concentrations in the brain and adjustive behavior patterns (5) as representing, perhaps, a partial token payment on my many outstanding promissory notes.

ogy" (read "molar psychology") remain probabilistic in nature or in principle. But if that is so, then if we are ever to build *theory* in behavior, we must, as Feigl (2) has expressed it several times, stop being "psychologists" (again read "molar psychologists"). In other words, since molar psychology is doomed to probabilistic relations, then *if theory is our aim* (and it need not be), let us be done with isolationist molar psychology (psychology *qua* psychology, behavior *qua* behavior), let us be done with sociology *qua* sociology, anthropology *qua* anthropology, let us not be frightened by that awful word "reductionism," and let us seek really to become unified scientists by consciously espousing the "microscopic" approach.

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FUNCTIONALISM, PSYCHOLOGICAL THEORY, AND THE UNITING SCIENCES: SOME DISCUSSION REMARKS

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Tolman and Brunswik (1, 24, 25), perhaps more deliberately than any other psychologists in the more recent development of the functionalistic approach in psychology, have emphasized the mutually substitutable nature of the mediating processes which occur between the environmental situation (stimuli, cues, means), and the responses (or achievements) in the behavior of organisms. Brunswik, with sovereign and perspicacious knowledge of the history and the systems of psychology, has illuminated in several publications (see especially 2, 3) the relationships of various ways in which scientific research may "focus" upon the subject matter in the biological-psychological area. The "thematic" and "tactical" differences of these various determinations of the type and level of research are not only compatible with the logical empiricist conception of the unity of science (for an exposition see Carnap, 5; Feigl, 6, 7), but actually support the conception of a unitary science precisely because of the obvious relationships between the assorted possibilities of focusing. It seems to me indeed only a tactical (if not a historical-terminological) question just what type or level of analysis one wishes to reserve for *psychology*, and which other focusings one wishes to delegate to biological ecology, physiology, neurology, or sociology and anthropology.

The mutual substitutibility ("vicarious functioning") of the mediating processes in organisms is indeed a striking feature. Biological thinking has borne its imprint for a long time. There is relatively little in the inorganic parts of nature (but of course a good deal in such inorganic artifacts as the servomechanisms) which invites the sort of concept formation which is so prevalent and fruitful in the bio-psycho-social sciences. Some imaginative scientists (not to mention several excessively imaginative philosophical cosmologists) have perceived principles of organization even in stars and atoms. But while Pauli's exclusion principle might indeed be viewed as basic to chemical organization, and Köhler's physical *Gestalten* (14) may describe a great deal of inorganic and organismic patterning, "vicarious functioning" or "means-ends" relationships seem primarily characteristic of the biological level (and everything above it in the pyramid of the sciences)—and of course in man-made machines.

Since scientific research zealously looks for regularities—be it for the sake of prediction, explanation, or control—it will fasten upon any sort of lawful relationships wherever it can find them. Regularities, however, are of many various forms and types (see Feigl, 10); there are those which we (rather sanguinely) consider *"basic,"* viz., the regularities formulated in the fundamental laws of theoretical physics; and there are other regularities which are obviously dependent on the structure of the systems (e.g., drops, bubbles, crystals, geysers, machines, organisms, social groups, etc.). Phenomena of homeostasis (e.g., regarding the relative stability of the blood temperature or the blood sugar level, etc., in mammals) provide a good example of regularities which depend on the structure (and

many functions) of living organisms. The regularities of perception, learning, and motivation as studied in behavioristic psychology likewise depend on the "intact" structure and functioning of the organism. If the "life," i.e., the rise and decline, of fixed stars as studied in astrophysics had as much "teleological" and developmental regularities as does the life of organisms, we would probably have a scientific "ecology" and "theory of adaptation of the stars," as we have ecological, evolutionistic, and developmental biological and psychological disciplines.

Brunswik knows, perhaps better than anyone else, that the subject matter of a science can be "carved out" in a number of ways. Ultimately there is only one criterion by which scientists decide which ways of "focusing" are preferable: "By their fruits ye shall know them." But what sort of fruits are desirable depends on one's interests. Skinner's (22, 23) fruits do not satisfy Krech's (15) appetites—and vice versa. Skinner's interests are focused on prediction-for-the-sake-of-control; Krech's on understanding and explaining. Skinner, like many a positivist before him, has his eye especially on the practical applications of science. The systematic design of his experiments selects certain narrowly circumscribed segments of behavior and examines them under various conditions of drive, stimulation, etc. The laws which he establishes, e.g., regarding response frequencies, are thus highly reliable, though of course statistical, i.e., not strictly deterministic. Brunswik's own research on size constancy in visual perception, to which he referred in the present symposium (4), proceeds by representative sampling (thus achieving a "close-to-the-actual-life-situation character") and thus also results in statistical laws; but it is as little concerned with a detailed theory of the mediating processes as is Skinner's psychology of the "empty organism." The more ambitious attempts of Tolman, Hull, Spence, and others to derive behavior regularities from various systems of theoretical postulates strive for more comprehensive unifications than Skinner or Brunswik in their respective (and different) procedures could ever achieve. But the success of molar behavior theories is still quite problematic, even if for the present it is more distinct than that of neurophysiological theories.

In view of the controversial question of the fruitfulness of the more highly theoretical approaches, one can readily understand why Brunswik tends to limit the subject matter of psychology to the study of certain (statistically certifiable) teleological relations between environmental and achievement variables. There is much in the common life connotations of the term "psychology" which would justify this delimitation of subject matter. But Brunswik, in so restricting his enterprise, cannot plausibly claim to produce a psychological *theory*. As I have argued at some length elsewhere (9), it may well be that sooner or later we shall have to recognize that explanations of the mediating processes can be given fruitfully only in terms of neurophysiology. In order to prevent misunderstandings I wish to say, however, that I should be very happy (and perhaps not too surprised either) if typically *molar* behavior theories were destined to succeed better than they have thus far. After all, classical thermodynamics was, and still is, a very powerful theory—independently of its (even more powerful) molecular interpretation. Here, with some reservations, I share the hopes expressed by Postman (21) and Hilgard (13).

The teleological patterns of perception, learning, and action may lend themselves to explanations of more or

less unitary form on the molar level. But an explanation of the very functioning of teleological mechanisms cannot be teleological itself (Nagel, 20); it will no doubt have to be of a relatively micro- or molecular character. This, I take it, is the lesson to be learned from cybernetics. The unification of the sciences which is being achieved along these lines is indeed much more exciting, though admittedly more problematic, than the merely methodological or empirical unity of science. With the abandonment of the earlier (logical positivist) thesis of the *translatability* of scientific theories into observation language, the more ambitious thesis of *unitary* science becomes again more vital and pertinent. The empiricist requirement, as we understand it today, demands only that the nomological net of scientific concepts and propositions be tied to observables in certain points. More accurately, the primitives of scientific theories are given their meaning by implicit definitions, i.e., by the postulates that combine them into a network with one another and with the observables.

Operationism, once helpful in eliminating metaphysical elements from the scientific enterprise, has tended to overstep the limits of its usefulness. There is, and here I agree with Krech (15, 16), in this day and age no longer any reason to be afraid of hypotheses. But (as I wish to remind Krech) it is of course wise to label as "promissory notes," "hopes for theoretical postulates," or "explanation sketches" what in present day neurophysiology can be regarded as no more than a program or blueprint for the derivation of behavior laws. Radical operationism has tended unduly to restrict psychological concept formation to purely "intervening variables" (see MacCorquodale and Meehl, 18), i.e., to dispositional concepts which are merely shorthand for empirical laws. It is questionable whether there are any important concepts at all in psychological theory which can be adequately conceived as pure intervening variables in this sense. (For a suggestive discussion of "conventional constructs," i.e., concepts which are neither pure empirical dispositionals, nor physiological existentials, see Lindzey, 17.) It will not help much either if intervening variables are construed as "open concepts," i.e., as dispositionals introducible by sets of test-condition → test-result conditionals open to additional (partial) specifications of meaning of the same (conditional) form. It is the distinctive mark and advantage of any *theory* with genuine explanatory power that it enables us to *derive* the total set of those conditionals. What would otherwise remain a miscellaneous collection of empirical laws or correlations thus acquires the sort of unity which only a theory can engender. Existential hypotheses or conventional constructs thus play a vital and indispensable role in theory construction; the atomic hypotheses in physics and the neurophysiological hypotheses in the behavior sciences are in many ways analogous to one another as regards their unifying power (see Feigl, 8).

Unitary science, understood in the sense of nomological nets (tied to points of observation), has no sharply separable parts or provinces. Only practical divisions of labor, convenient lines of demarcation of research, can be recognized as separating psychology from biology or physiology on the one side and the social sciences on the other. Even if Brunswik's own predilections in defining the subject matter of his probabilistic-functionalistic psychology prevent him from attaining the theoretical level in his research, he has—in the capacity of a metatheorist (i.e., as a comparative methodologist of science)—provided extremely valuable suggestions

regarding the place of psychology in the system of the united (or rather uniting) sciences.

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IN DEFENSE OF PROBABILISTIC FUNCTIONALISM: A REPLY

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There are two aspects of probabilistic functionalism: representative design and functional theory (11). Let me consider the arguments concerning theory first.

1. As a starting point, let me take exception to the use of the term "uniformity point of view" in contradistinction to "probability point of view" and also to the identification of the former with the nomothetic approach with which Postman's cogent analysis (27) begins. Uniformity is a matter of reality, or of beliefs about reality, while the search for laws is a matter of what to admit to representation out of that reality. I suspect that one of the main sources of confusing the existential with the representational in the present context is the otherwise splendid book of readings on contemporary psychological theory by Marx (24) in which papers by Hull (20) and by myself (5) were rechristened and juxtaposed under the above labels by the editor. These papers had originally been part of a symposium in which Lewin (23), for once, joined forces with Hull if only in common defense of the nomothetic point of view against my own plea for probabilism.

The term "uniformity point of view," which I myself have never used, erroneously suggests that being a probabilist excludes the belief in uniformity, perhaps in the way in which some of the quantum physicists—or in quite another way the vitalists—do not believe in uniformity. The allegation which Hull made in the same vein at the beginning of his paper, asserting that I do not believe in the "existence" of laws applicable to behavior, is thus based on a misunderstanding. The same holds for the present remarks of Krech (22). Contrary to what he imputes, I *do* believe that "God does *not* gamble"; I fully realize that the impasse of quantum physics is irrelevant to our case, uncertainty being for all practical purposes wiped out at the level of physical macrolaws—which are still microlaws to us psychologists—and that vitalism is mere dogma.

But the crucial point is that while God may not gamble, animals and humans do, and that they cannot help but to gamble in an ecology that is of essence only partly accessible to their foresight. And although an infinite and omniscient intellect could operate by law and ratiocination alone, as a psychologist even such a being would have to follow the methodological postulate of behavior-research isomorphism (10, p. 25) and operate at the probabilistic level of discourse. As in geography or textural ecology, this does not entail the negation of law; it merely entails relinquishing attention to law within certain contexts. As these disciplines do not go "beyond" law, we do not either; and as they are sciences, so is probabilistic psychology.

As we all recognize, I presume, not only the legitimacy but also the necessity of the strictly descriptive or statistical disciplines in addition to the nomothetic sciences, it would seem that Krech's as well as Feigl's (15) tendency to relegate the definition of the basic thema of a science to personal preference and taste amounts to a shrinking away from the tasks and obligations inherent in the scientific potential of reality. Each level of parsimony and of

generalization, from the most disjointedly enumerative description to the most universal of laws, entails a challenge of its own. As there must be a balance of occupations in a society so that all the potential demands are filled, so in science at large there must be an equitable distribution among the variety of potential themata. Insistence on reduction as a universal goal of science can only result in blighted spots on the landmap of scientific enterprise. Rather than excommunicating functional psychology from the sciences, advocates of this exclusive form of reductionism—be it persons or groups or even entire phases of the history of our science—are embarking on a self-excommunication from psychology.

Probabilistic functionalism is not, in turn, hostile to reduction. It merely places correlational achievement mapping of generalized functional arcs at the top of a hierarchical pyramid (10, ch. ii); this is followed by the macromediational analysis of vicarious attainment strategy. As to the third level in this pyramid, the reductive study of micro-mediational tactics, I agree with Professor Hilgard (19) that this and the entire nomothetic approach should not be "replaced" by the probabilistic approach of the two top levels, although I would like to place reductionism in a marginal position to psychology unless it is executed in firm contact with the two functional aims. In order to reduce, we must know what to reduce. We must reduce "from above," that is, starting from such high-complexity functional units as the lens model. Bertalanffy (1, 2) has in effect done so when he attempted a reductive explanation of "equifinality" in terms of Prigogine's new open-system thermodynamics (see also 10, pp. 42 f.). Most of the "laws" that play a part, say, in learning theory were gathered at a surface level of complexity, and either in oblivion of, or in but casual or mysterious contact with, the more broadly functional contexts in which they are embedded.

The nomothetic-reductionist-systematic type of approach has in the past been overstressed at the expense of the probabilistic-functional-representative approach, notwithstanding all the epoch-making discoveries to which Hilgard has referred. We have had all of the former and nothing of the latter for too long. Now we must balance psychology in the molar and molecular realm.

In contrast with most previous movements in psychology, probabilistic functionalism thus does not exclude anything; rather, its scope is encompassing enough to allow us to envisage a system to end all systems.

2. We have recently been admonished by Koch (21) to expand the use of the label "theory" beyond the nomothetic endeavors in the narrower sense. What Koch, and probably Feigl in the present symposium, mean by "metatheory" is theory about theorizing, perhaps including matters of differential thema and design as subsumable under the label of comparative methodology.

But it seems to me that the liberalization of our notions of theory must go still further. We must include within the scope of theory proper the substantive part of functional considerations also, involving primarily the structure of the lens model as a ratiomorphic explication of achievement rather than merely its reduction to physiology or physics. This model involves focal points, areas of unspecificity, mechanisms of substitution, and other devices of multiple mediation. When Feigl demands generality of a theory, the lens model has it: in fact, the very essence of generalized achievement is incorporated within it, and it can be transferred from perception to action and

other psychological functions. It even may be used for "explanation" in the sense of subsuming a single act under a generalized "wish," ascertained by the probabilistic application of operational criteria of "purposiveness" such as those of Tolman (see 29, ch. i). The lens model also has productivity: predictions concerning the efficiency and foolproofness of functional attainment, concerning performance in other areas or dimensions of the ecology, and so forth, may be "derived" from it, and the probabilistic character of these derivations should not detract from the potential rigor and objectivity of the procedures involved.

In other words, we must restore the term "model" to its original meaning, as in speaking of a model of San Francisco Bay water currents, or of the models of medieval technologists. In speaking of "models," many psychologists have come to confine themselves to what essentially amounts to mathematical curve fitting in the tradition of strictly nomothetic theorizing; as we have seen, however, this not only is a labor of Sisyphus but also a labor the fruits of which may be blown over like a house of cards when representative design is applied.

Professor Feigl's reluctance to part with the cherished technical meaning of the term "theory" is, to be sure, reinforced by the majority trend in recent psychological "theory construction." But we must insist that the opening up of probabilistic vistas requires some flexibility, some of the "positivism, not negativism" which he himself has preached in other contexts (13) but which, as it must seem from his remarks, he has not always exercized when the challenge was to recognize the growth potential of the psychological thema. This technical meaning of theory, incorporated as it is in Feigl's hierarchy of "levels of explanation," which stretches from strict description through surface laws to low-order and high-order theory (14), reveals only too clearly its origin in the situation in physics with its reduction of, say, the low-order theory of planetary motion by Kepler to the higher-order theory of gravitation by Newton and eventually to the theory of Einstein. Trying to enforce this content-bound schema on another domain is but one more instance of thematic physicalism. Like all automatic transfer of content-alien scaffoldings, it can only result in obscuring the indigenous thema and theoretical structure of the host discipline.

That the question of the applicability of the term "theory" cannot be dismissed as a mere matter of terminology became clear to me when I was asked, at Gustav Bergmann's instigation, to write a note on one of the less well-known papers by Hammond, one in which the situation created in psychology by representative design was set in analogy to that in relativity physics, with special reference to the fact that in both cases regularities hitherto considered universal turn out to be contingent upon a limited ecology (18). Bergmann saw a difficulty in the fact that it was a "design" that was juxtaposed to a "theory." In my reply to this argument (9) I pointed out that the methodology of representative design was inseparable from an inherent theory, and that this theory had been made sufficiently explicit in the original publication (7) under the more modest label of "generalization." The same policy has in part been followed in my main contribution to the present symposium (11).

3. If I understand correctly, Krech agrees with the general thesis that the nomothetic and the microscopic approach go together, while Postman does not. The latter correctly points to Skinner as a psychologist who is nomothetic yet nonmediational. But micromediationism, reductionism, or physiologism are but a few of many possible forms of being atomistic. Another is the encapsulation within the boundaries of the organism, of which Skinner is equally guilty with Hull, and which is

the price paid for some success along nomothetic lines.

It also must be held against Postman that only if our aim is microexplanation or nomothesis at large must we depart in our investigation from the conditions which we intend to explain. The same holds with respect to what Feigl has pointed out in referring to Nagel's telling objection that explanations of teleology cannot in themselves be teleological. The aim of functional psychology is indeed like that of applied psychology, as Postman correctly imputes; or perhaps it goes even further than applied psychology, which has employed systematic design against its own best interests.

4. And now to Hilgard's penetrating argument on representative design as it affects our sampling study of size constancy (6). His suggestion of reading the names of the objects to a blindfolded subject and obtaining estimates of size on this basis will be followed through. Let me venture a guess as to the outcome. In our ecology the functional achievement correlations will be still high, although appreciably lower than in the original study with its unhampered use of the common direct cues. But it is not true that with the use of words we are necessarily overstepping the boundaries of perception in an uncontrollable way, as Hilgard seems to imply. On the contrary, perception must be seen as a cross-departmentally unitary function, and words or other auditory cues are as legitimate a part of the potential mediating cue family hierarchy as are the visual—and in fact the tactile-kinesthetic—cues. Since object names had to be used by the subject of the original study in her communication with the recorder in the course of the observations, Hilgard's suggested design falls more in the category of impoverished rather than in that of shifted cue situations. Under fully representative design, it would be a violation of basic policy *not* to include name cues and the analysis of their effects, rather than the contrary as Hilgard seems to assume.[1]

One must not forget that, ideally, representative design would require huge, concerted group projects, involving as it does a practically limitless number of variables, but that at the same time its rewards in after-the-fact evaluation could be equally widely ramified. In the pilot study under discussion (6) only a select few of these evaluation possibilities were exploited, and the entire problem syndrome of cue participation was left in the dark save for the indirect ascertainment, via the sampling of the objects, of a reasonable degree of representativeness. Since, in principle, under representative design all variables are allowed to vary and none are held constant artificially, their role can be ascertained after the fact. For the same reason, there is a gain rather than a loss of information, contrary to what Postman seems to fear. And we may add that partial correlation, on which Hil-

[1] As this goes to press I have before me an as yet unpublished, independently conceived study by R. C. Bolles and D. E. Bailey in which a representative sample of $N = 54$ familiar objects was presented to $n = 5$ adult subjects first nonvisually by naming or brief verbal description, and then visually. The authors have kindly given me permission to quote from their results. Nonvisual estimates were almost as accurate ($r = .988$) as visual estimates ($r = .994$), and in fact considerably more accurate than this writer would have guessed offhand. Yet the difference between the two achievements was found to be statistically significant (using the ecological N of 54, as is called for under representative design); in addition, the errors under the two conditions correlated to a relatively moderate extent only. The cue systems involved in the two performances thus seem to a considerable extent nonoverlapping, perhaps having less in common than I conjectured when I suggested above that the verbal situations were merely impoverishments of the full perceptual situations.

gard has expressed reservations, is just one example out of many possible mathematical ways open to, or to be developed by, the statistician for this after-the-fact analysis.

Our earlier prediction of the outcome of Hilgard's suggested naming experiment had to be specified to our ecology. Another ecology is conceivable in which the common objects sampled, such as books, windows, trees, hills, and so forth would be so univocally standardized that, given proper perceptual learning, the name could be utilized as a foolproof cue to size. Hilgard's variant of our design would in this case yield perfect correlation, while visual cues could merely upset this achievement. With such a change in ecology the trend of results would thus be reversed, much as in Hilgard's example from Sears and Whiting there was a trend reversal which likewise could be subsumed under representative design.

I should like to add parenthetically that this latter example seems to me to involve primarily a responder-populational variable—weaning age and its range—and only secondarily an ecological impasse; but since both such types of universes should be treated alike and sampled under representative design, this difference is fundamentally beside the point here.

5. A similar expansiveness applies to the case of Postman's concern lest motivational bias be excluded from representative design. Representative design merely complements and completes the existing ways in which the necessity of representative sampling has been recognized, so that in the end such sampling will extend over all psychologically relevant variables.

This does not imply, however, that the concept of functional research should be equally broadly conceived. Postman is correct in assuming that the "veridicality" of perception is the major issue in the study of its functional validity, and that our brand of functionalism is not directly and not primarily concerned with need variables or the structure of the need system. Only insofar as needs or values help the cause of, or interfere with, veridicality—and this may be the case either in "perception" or in "judgment," as Postman correctly remarks—do they become part of the functionalist's direct concern. Even interference may be functionally constructive, such as when needs serve as emphasizers of the size or of some other attribute of a life-relevant object. Furthermore, as we have said (11), values form part of the system of natural-cultural "laws" and as such in themselves become objectives of cool, veridical attainment; it is primarily for this reason, and not because of their entanglement with the world of needs, that we incorporated them in our earlier perceptual constancy program (4). And we may add that all these variables are as much a potential part of an isolating statistical analysis as are Hilgard's word cues.

It is only when it comes to delving into the need structures themselves, their variability and their etiology, as is the case, say, in Bruner and Goodman's comparison of the effect of values on rich vs. poor subjects (3), or in Postman, Bruner, and McGinnies' development of such dynamic aspects as "perceptual defense" (28), that the wisdom of applying the label of functionalism may be doubted.[2] This must be made explicit, especially in view of a somewhat unfortunate current trend toward an overexpansion of the concept of functionalism, by Osgood (23, pp. 294 f.) and by others, according to which all such need considerations would be seen as an outgrowth of the functionalist tradition. Bruner and Post-

[2] *Note by Leo Postman:* As I have tried to show elsewhere (26), the phenomena originally interpreted as perceptual defense can be adequately accounted for in terms of (*a*) principles of associative learning and (*b*) variables governing *Ss*' performance in the experimental situation. On these grounds the relevance of such phenomena to functionalist analysis in the narrower sense specified by Brunswik may be defended.

man can themselves not be entirely absolved of having contributed to this trend (for further discussion see 8). In genuinely functional analysis needs appear as mere background elements, only as necessary for all utilitarian considerations—that is, essentially as constant parameters rather than as actual variables or as systems of interdependent variables—the latter being exemplified by Frenkel-Brunswik's "personality-centered" approach to perception (17).

Let me conclude with a consideration of Postman's plea for the *status quo*. As has been pointed out repeatedly in this symposium, much depends on definition. Therefore, if it is being recognized that representative design and the resulting functional theory are a going and a growing concern, while systematic design and the nomothetic approach are about to level off and to reach the point of diminishing returns —if, in other words, the *status quo* is conceived not in a static but in a dynamic manner—then I too am heartily in favor of this *status quo*. And when I hear the acknowledgment, as I think I have by Postman and by Hilgard in the present discourse, that systematic design is a preliminary and an eye-opener to the attack on a larger, representative scale, then I am confident that this *status quo* will indeed be a dynamic one.

The peaceful coexistence of which Postman speaks may even be turned into a situation in which the probabilistic approach begins to repay the debt it owes to the reductionist vanguard. In discussing Hilgard's argument on representative design we have come across the problem of perceptual learning. The presence of such learning must be assumed on the broadest possible basis if perception is to be adjustment to an ecology. It is for this reason that an experimental study of the acquired utilization of artificial illumination cues in perceptual color constancy was made part of our Vienna program; the finding was that such acquisition is possible to a certain extent but that perceptual learning requires slow "stamping in" and is surprisingly autonomous of explicit intellectual awareness (Fieandt, 16).

Even purely ecological surveys may become fruitful for the theory of perception and learning. This writer and Kamiya (12) have demonstrated (with the use of $N = 892$ separations between adjacent parallel lines in a roughly "proportionate" sample of shots from a current motion picture) that the long-recognized gestalt factor of "proximity" possesses a certain modest ($r = .12$) but statistically significant ecological validity as an indicator of mechanical object unity. Its utilization as an organizing principle for perceptual "figures" is thus a probabilistically adjustive mechanism. The realization of this fact may help to open the door for a possible "reduction" of the hitherto unabsorbed gestalt dynamics into learning theory. The latter may thus eventually become universalized under the associationist-functionalist aspect, much as Newton's theory has incorporated both terrestrial gravitation laws and Kepler's laws of planetary motion within a common higher-order theory.

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